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Science Progress

A MONTHLY REVIEW

OF

CURRENT SCIENTIFIC INVESTIGATION

CONDUCTED BY

HENRY C. BURDETT

EDITED BY

J. BRETLAND FARMER, M.A.

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Science Progress.

No. 19. SEPTEMBER, 1895. Vol. IV.

PROGRESS IN THE STUDY OF THE ANCIENT SEDIMENTS.

IN the latter half of the year 1894, considerable additions were made to our knowledge of the ancient sediments, both at home and abroad, though no authenticated discovery of exceptional importance was announced. Somewhat sensational announcements appeared during that time, but the results of greatest value are due to steady work at points of detail, for these results are not given to the world until they have received ample confirmation at the hands of the workers. It is pleasing to find that British geologists have done their full share in carrying on work of this nature, so that year by year the "records of the rocks," both at home and abroad, become appreciably less imperfect.

Taking into account, in the first place, treatises which appeared during the second half of last year, which throw new light on more than one of the great divisions of the older rocks, we may notice at the outset the very important results obtained by various members of her Majesty's Geological Survey as recorded in the official report (1). It is there announced that the Torridonian rocks, previously proved by the surveyors to be of Precambrian age, are divisible into four groups, as follows in descending order :—

4. Cailleach Head Group, 1000-1500 feet sandstones, flags, shales, and calcareous bands.

3. Aultbea Group, 2000-3000 feet chocolate and red sandstones, flags, and shaley partings.

2. Applecross Group, 4000-5000 feet coarse arkose.

1. Diabeg Group, 5000 feet and upwards red sandstones, greywackes, shales, limestones, and calcareous bands. The Diabeg group is further divided by Clough into four subgroups.

In Ireland equivalents of the Precambrian and Lower Palæozoic rocks of Scotch type are gradually being mapped in detail. Much information is given concerning the Southern Highlands of Scotland, including the discovery by Barrow of radiolarian cherts in Forfarshire and Kincardineshire on the edge of the Highland border. Lamplugh has shown that in the Isle of Man the "Skiddaw slates" are everywhere folded, faulted, and sheared, and that the apparent great thickness of these rocks in the island is therefore deceptive, a conclusion which seems to bear out a similar statement previously made with regard to the "Skiddaw slates" of the Lake District. In the Isle of Man slates, coarse breccias are found, and these are considered by Lamplugh to be friction breccias. In Devonshire the Devonian rocks of the Plymouth area have been brought into connection with those of Newton Abbot and Torquay. One of the most interesting announcements in the report is that explorations in Cheshire and South Lancashire "appear to prove that in both counties the Lower Mottled Sandstone, hitherto taken as the base of the Bunter, belongs to the Permian series, for above it, and below the Bunter pebble beds, lie marls with *Schizodus*". No doubt all these points will receive fuller treatment in detailed memoirs, and indeed a paper has already been read, though not yet published, on the breccias of the Skiddaw slates of the Isle of Man.

Passing from Britain to her possessions, it is interesting to have the mature judgment of the veteran Sir William Dawson on various disputed points (2). Concerning the Precambrian rocks he remarks that "the older gneissic group of the West Highlands of Scotland does not contain the whole of the Laurentian of Logan, the Lewisian of Murchison, but only or mainly the lower part of it, the Ottawa group of the Canadian Survey. A certain limited

tract at Loch Maree not improbably represents the Upper Laurentian or Grenville series, and this certainly occurs in the western islands. . . . The Torridon sandstones and the associated beds . . . seem . . . to be equivalent to the Huronian of Logan. . . . The Uriconian and Longmyndian of Geikie probably include the equivalents of our Kewenian, and the same may perhaps be said of the Dalradian of Scotland. . . . Some portions of these rocks may, however, be the same with what in Canada has been called by Matthew 'basal Cambrian'." Without commenting further on the vexed question of the correlation of the Precambrian rocks of different areas, which will remain in a state of uncertainty until far more work has been done amongst the rocks, we may pass on to notice a sentence occurring in Sir William's criticism of the theories of coal-making: "The coal-deposits of the great Carboniferous system . . . mark a special stage in the production of our continents, when they were less differentiated as to orography, and when a very uniform and equable climate extended over the northern hemisphere accompanied by a very peculiar vegetation. Such conditions did not occur in combination and to a like extent in any succeeding period in the earth's history." This sentence contains a note of warning which some writers will do well to consider. Extreme uniformitarians sometimes appear to forget that in every system, differentiation has proceeded to a greater extent than in the preceding one, and that as with the Carboniferous so with the other systems, the conditions do not "occur in combination, and to a like extent in any succeeding period".

Having passed over the Atlantic we may pause there before returning to the Old World to notice two papers treating of kindred subjects, the occurrence of certain conglomerates amongst the Palæozoic rocks. The first of these treats of "Palæozoic Overlaps in Montgomery and Pulaski Counties, Virginia" (3).

The rocks described in this paper are in ascending order:—

1. *Cambrian.* The Graysonton Formation, a mass of

red and green shales and interbedded limestones generally siliceous, containing no fossils; their exact age is therefore as yet unknown.

2. *Cambro-Silurian.* Shenandoah Limestone, 4000 feet thick, generally a grey dolomite much folded and crushed, at the top are a few feet of blue sparingly fossiliferous limestone, probably the only representatives of the Trenton-Chazy limestones in this region. The limestones contain conglomerates with pebbles similar in character to the matrix. With reference to the nature of this conglomerate, the writer states that "its meaning is obscure".

3. *Silurian.* Sevier (Hudson River Shale) contains fossiliferous limestones. It is not quite clear why this formation is placed by the writer in the Silurian, if he uses the term "Cambro-Silurian" in the sense of Ordovician. It is succeeded by the Church (Medina-Oneida) sandstone, the fossiliferous Rockwood (Clinton) formation, and the Giles (Lower Helderberg and Oriskany) formation (the Oriskany and all save the lowest subdivision of the Lower Helderberg are usually included in the Devonian).

4. *Devonian.* Walker Black Shale and Kimberley Shale.

5. *Carboniferous.* Price (Pocono) Sandstone closely resembles the upper Coal Measures of the Appalachian basin and is succeeded by the Pulaski shale.

The author describes five periods of Appalachian folding, the first being in Lower Cambrian times, the second near the top of the Shenandoah limestone, the third which was the main period of deformation occurring between the Devonian and Silurian periods, the fourth near the middle of the Carboniferous, and the fifth, post-Carboniferous. The last has been generally considered the most important of the Appalachian foldings, though "it now seems as though this was no more important than many which preceded it, and that in fact the deformation has been practically continuous since early Palæozoic time".

The second paper to which allusion has been made is on "Palæozoic Intra-formational Conglomerates" (4). In this various palæozoic conglomerates are described, with pebbles

which often contain a fauna similar to that of the matrix in which the pebbles are included. These chiefly occur in the case of limestone. The author observes that "the history of Appalachian sedimentation and mountain-building proves that a more or less constant movement was taking place from Algonkian[1] time to the close of the Palæozoic. This movement was at times greatly prolonged and resulted in marked topographic features. More frequently the minor movements produced local effects, and some of them resulted in the formation of the conglomerates described."

These papers have been noticed at some length because they appear to confirm results obtained by geologists in the Old World that orogenetic movements occurred during much longer periods of time than was once supposed, though each great uplift was not necessarily the result of continuous movement. At the same time it is not clear that all the conglomerates described in these papers are true deposits. The writer of this article has had frequent occasion of late years to study friction-breccias and friction-conglomerates, and he finds that the latter are specially prone to occur in limestones. In such cases the occurrence of a similar fauna in the matrix and the pebbles would be readily explicable. The statement of one of the above authors that the meaning of one of the conglomerates "is obscure" encourages one to suggest the probability of the occurrence of friction-conglomerates amongst the highly disturbed rocks of the region described, for they are far from rare amongst the disturbed regions of Europe, and frequently simulate conglomerates of deposition with extraordinary fidelity.

A paper by M. Marcel Bertrand (5) on the Alps is chiefly devoted to the foldings and their effects, but contains information about the Alpine Carboniferous rocks. Another Alpine paper (6) treats of a district which has previously been described at considerable length by Lory; in this paper also the systems of folds are discussed in detail. A

[1] Late Precambrian.

description is given of the Precambrian rocks, consisting of hornblendic gneisses, amphibolites, and mica-schists (both granulitic and non-granulitic). Carboniferous rocks (grès à Anthracite) appear on each side of the principal ridge in the form of two regular synclines (an eastern and a western), which seem to correspond with two separate basins occupying depressions on each side of a precarboniferous chain. Some of these Carboniferous beds have suffered considerable metamorphism, and the author gives petrological descriptions of the macroscopic and microscopic characters of a number of new types of these metamorphic rocks, including highly altered conglomerates, sandstones, etc. In most cases sericite is the only new mineral produced by the metamorphism. Orthophyric volcanic rocks are described; stratigraphical observations lead the author to consider that most of the lavas occur at the top of the Carboniferous deposits of the Grandes-Rousses Massif. A description of the systems of folding is given, the Hercynian folds run generally north and south, as does one set of the Alpine folds, whilst another set of the latter runs east and west. The paper is a valuable contribution to the effects of dynamic action in producing metamorphism of recognisable sediments.

Taking now into account the papers treating of special deposits or groups of deposits, we do not find many dealing with important questions concerning Precambrian rocks. Bell (7) describes some remarkable outliers of Potsdam beds and Black River limestones in depressions of granite. He suggests that these depressions are due to submarine weathering. Much of the Archæan rock must have been denuded before the deposition of the Palæozoic rocks upon it, and the interval between them is considered to have been probably much greater than has generally been supposed. It is to be observed, however, that the presence of Black River beds immediately succeeding Precambrian rocks, indicates further weathering of these rocks in Palæozoic times before the deposition of the Black River limestone.

Wiman (8) describes as organic a problematical body

from the Visingö group which lies between the "Urgebirge" and the Lower Palæozoic rocks of Scandinavia. There is no doubt, as has been before remarked, that Precambrian fossils will be found to exist when these rocks have been more fully studied, but it is very necessary, considering the many occasions on which the existence of these fossils has been maintained on insufficient evidence, that very conclusive evidence should be brought forward before the actual discovery of Precambrian fossils is taken as generally established. At present the evidence for the existence of Precambrian fossils is very much in the same condition as that of the discovery of human bones in palæolithic deposits. It is generally admitted that man existed in palæolithic times from evidence other than that of the bones, some of the human bones that have been claimed as of palæolithic age may possibly, nay, probably, be of that age, whilst the greater number of them certainly are newer, and in the opinion of many competent authorities the actual proofs of the palæolithic age of any one human bone remain to be given. So with Precambrian fossils, the only difference being that in the case of the palæolithic period we have actual evidence of the existence of man furnished by implements, whereas in that of the Precambrian fossils, the existence of organisms in Precambrian times is inferential, the great complexity of the earliest Cambrian (*Olenellus*) fauna rendering it practically certain that earlier faunas existed on the earth. It will be remembered that in addition to the case described by Wiman, the existence of Precambrian fossils has been recently asserted in the phyllades of St. Lô and the Torridonian of the Scottish Highlands. The fossils stated to occur in the latter beds have not yet been described, and, as noted in a previous article, doubt has been thrown on the organic nature of those from the former. Perhaps the least doubtful of the many recorded instances of Precambrian fossils is that about to be noticed. I refer to a paper by Noetling (9). He describes the following beds in descending order :—

4. Bhanganwalla group, or salt-crystal pseudomorph zone.

3. Jutana group, or Magnesian sandstone.
2. Khussak group, or *Neobolus* beds.
1. Khewsa group, or purple sandstone.

The whole forming the Punjab "province" of Cambrian beds. The purple sandstone is stated to be not clearly separable from the *Neobolus* beds. The latter consist of dolomitic sandstone alternating with shales, and have a thickness of from twenty to one hundred and fifty feet. They are split up into five subdivisions in descending order :—

E. Zone of *Olenellus* sp., containing also *Trimerellidæ*.
D. Zone of *Neobolus Warthi*.
C. Upper Annelid sandstone.
B. Zone of *Hyolithes Wynnei*.
A. Lower Annelid sandstone.

He maintains that he confirms Waagen's view that the fauna of the *Neobolus* shales is explicable by the fact that the fauna is older than any known Cambrian fauna, occurring as it does below the *Olenellus* beds, in which case *Neobolus Warthi*, *N. Wynnei*, and *Hyolithes Wynnei* must be considered the earliest known forms of animal life. The Magnesian sandstone is separated into five subdivisions on lithological characters. It contains *Stenotheca* and is believed to be Lower Cambrian. No fossils have been found in the salt-crystal pseudomorph group.

A few observations may be offered on the facts contained in this paper. In the first place, the concurrence of two authorities of such repute as Waagen and Noetling in maintaining that the *Neobolus* beds contain a fauna older than any known fauna is of very great importance, nor is its importance lessened by the fact that these authorities do not altogether agree concerning minor points. At the same time, it must be pointed out that the evidence is of a negative character. *Olenellus* has been found in the uppermost subdivision of the *Neobolus* beds, but not in the lower ones, still it is quite possible that the genus and its associates may eventually be found therein, especially when we take into account the small thickness of the group, and the similarity of characters of the different divisions which is

sufficiently great to allow them to be included in one group. At least two horizons of *Olenellus* are known elsewhere as proved by Schmidt in the year 1888, and considering that the *Olenellus* group is comparable in importance with the *Paradoxides* group, which is divisible into several zones characterised by different species of *Paradoxides*, as established by the researches of Hicks, Brögger, and Tullberg, and also that so many species of *Olenellus* are now known, we may well suppose that the *Olenellus* beds like those containing the *Paradoxides* fauna may be separated into several zones characterised by different species of *Olenellus*. It has not yet been made clear that the *Neobolus* beds are not the equivalents of some of these, for the fossils associated with *Olenellus* have not yet been described from all the areas where *Olenellus* is found.

Should the *Neobolus* beds be definitely proved to be lower than the *Olenellus* beds, the question as to where the base of the Cambrian system should be drawn, which is bound to confront us sooner or later, becomes a matter of pressing importance, for it will not do to absorb group after group into the Cambrian rocks as they are found to be fossiliferous.

The Cambrian system as defined by Sedgwick is typically developed in North Wales, where its base is formed by the Harlech and Llanberis beds. In these North Welsh beds the *Paradoxides* fauna is known to occur, but the existence of strata of the age of the *Olenellus* beds is only inferred, no definite fossils of *Olenellus* age having yet been detected therein. Still, the *Olenellus* beds have so constantly been referred to the Cambrian system that it would introduce additional confusion to relegate them to an earlier system, should it be ultimately proved (which is unlikely) that none of the beds of Sedgwick's typical area appertain to the beds of *Olenellus* age. Also considering that the *Olenellus*, *Paradoxides*, and *Olenus* faunas are of approximately equal importance, the beds containing them make a convenient system as the term is understood amongst geologists. But the inclusion of beds of earlier age than the *Olenellus* beds (such beds being probably not represented in North Wales)

in the Cambrian system is to be deprecated, and the writer would suggest that any fossiliferous margins which are in future proved to be of an age antecedent to that of the *Olenellus* beds should be placed in a system earlier than the Cambrian.

A paper of interest to British geologists (10) adds to our knowledge of the ridge which existed in Central England in early Carboniferous times. Prof. Hull records the results of a boring which passed through 262 feet of Bunter Sandstone and 514 feet of Coal Measures, and reached a reddish-purple and grey grit and micaceous quartzite which was pierced to a depth of 19 feet from its summit. He considers that this probably belongs to the Lower Cambrian, a view with which Prof. Lapworth concurs, in which case the Lower Coal Measures and all the Carboniferous rocks beneath them, as well as the Devonian, Silurian, Ordovician and higher Cambrian rocks, are absent.

A valuable historical sketch, treating exclusively of the Cambrian rocks, appeared in the latter half of 1894 (11). In this paper Dr. Hicks gives an account of the progress of work during thirty years as concerned with the discovery of the various life-zones of the Cambrian rocks. Though the paper is a historical sketch, and not a record of new observations, it is of importance to the student not merely as an epitome of our present knowledge of the Cambrian strata, but also as an indication of how that knowledge was obtained, and is therefore particularly suggestive of lines along which research may be carried in the immediate future.

Several contributions to the palæontology of the Ordovician and Silurian rocks have been made in the period under notice. Počta (12) gives another instalment of the great work commenced and carried out for so many years by Barrande; he describes the Bryozoa of the Lower Palæozoic area of Bohemia. F. Schmidt (13) makes another contribution to his accurate descriptions of the trilobites of Esthonia and Livonia. In this memoir he treats of the genera *Calymene*, *Bronteus*, *Proetus*, *Cyphaspis*, *Menocephalus*, *Harpides*, *Harpes*, *Trinucleus*, *Ampyx*,

Remopleurides, and *Agnostus*. He adopts three groups for the genus *Calymene*, *viz.*, *Calymene* proper, *Pharostoma corda*, and *Ptychometopus* n. subgen., and gives diagnoses of them. T. S. Hall (14) has a "Note on the Distribution of the Graptolitidæ in the Rocks of Castlemaine (Australia)". Some of the identifiable forms, according to the author, range through the series, whilst others afford a ready means of distinguishing certain facies. As *Tetragraptus* ranges throughout the beds, they would seem to be of Arenig age notwithstanding the occurrence of "what is apparently a *Monograptus*". The forms recorded are similar to those of the Skiddaw slates of England and the Quebec group of Canada, and appear to be restricted to certain horizons of the strata. With regard to the apparent *Monograptus*, it may be suggested that it is possibly an *Azygograptus*—*Monograptus* it certainly cannot be if it be associated with the graptolites occurring in the strata throughout which *Tetragraptus* ranges. The fact that certain species appear to be restricted to definite horizons of these *Tetragraptus* beds is of interest, for until quite lately the existence of definite zones in the corresponding Skiddaw and Quebec beds, both of which have undergone much disturbance, was doubted.

Consideration of the contributions to our knowledge of the Upper Palæozoic or Deuterozoic (Devonian, Carboniferous, and Permian) beds, which were made during the second half of the year 1894, must be left for a future article.

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JOHN E. MARR.

ON THE RESPIRATORY FUNCTION OF STOMATA.

THE part played by the stomata of a leaf in the gaseous exchanges essential to respiration and assimilation has been much debated, and at the present time leading text-books express entirely opposed views. Yet it would seem to be a question of primary biological importance whether the vast quantities of carbon dioxide that are taken up by assimilating green leaves enter through the stomatic openings or are absorbed by the much more extensive, cuticularised, exposed surfaces of the epidermal cells. Until this is decided no very definite conception of the physiological significance of the leaf structure is possible.

It is proposed to give in this article, firstly, an historical sketch of the conflicting theories and experiments that have been advanced in the attacks on this problem; then a brief account of a complicated apparatus that the author has designed, capable of obtaining direct experimental evidence from the living leaf on this function; and finally a *résumé* of the results so obtained, which it is trusted furnish a final solution of the problem.

The earliest paper that must be noticed is by Dutrochet (1), 1832. He draws attention to the extensive system of intercellular spaces to be found in leaves, especially in the lower strata, and demonstrates by injection the connection with the stomata. From determinations of the composition of the gas normally contained within these spaces he concludes that this internal air has a real physiological significance, and that the leaf of the plant has a lung-like function, handing on to the outer air the carbonic acid formed within the bulk of the plant, and returning oxygen in exchange.

Interest in the *rôle* played by cuticle, then newly investigated, in absorption and exhalation by the aerial parts of the plant, stimulated Garreau (2) in 1850 to carry out a series of researches on these points.

Among other results he arrived at the well-known one that transpiration of water-vapour takes place to a much greater degree through areas provided with stomata than through uninterrupted cuticle, so that more water is given off from the lower than from the upper surface in the great majority of leaves.

In the same paper in which these often-quoted experiments are detailed, there is a usually neglected section which bears immediately on the subject now under consideration. In this Garreau gives the results of the extension of his method to the comparison of the amounts of carbon dioxide given off by the two surfaces of leaves respectively.

He attached a glass chamber containing a small shallow capsule of lime water to corresponding areas on each surface of a leaf. The carbon dioxide liberated by the leaf produced a film of calcium carbonate, of varying thickness, on the surface of the lime water; this was too fragile for direct weighing, so he collected it on pieces of tared filter-paper, and after drying determined the gain in weight. His estimations are all too high, probably increased by CO_2 from the air, and the method is of course decidedly inadequate; but the results tended strongly to show some relation between the distribution of stomata and the evolution of carbon dioxide, and, among the conclusions to the paper, we find it stated that the expired quantities of carbon dioxide appear to be in a much closer relation to the distribution of stomata than do those of exhaled water. No attempt to obtain similar numbers has been published during the time that has elapsed between the paper under consideration and the present author's work.

In 1867, Boussingault contributed a long experimental paper (3). He directed his attention entirely to the gaseous exchange taking place during active assimilation. As this exchange far exceeds in intensity that of respiration, the former subject appears much the more likely to give a decisive experimental answer to the problem of the nature of the path of exchange.

The results, however, led him to take up a view exactly

the contrary of the one supported by Garreau, and to minimise the function of stomata in gaseous exchange. He appeared to prove by trustworthy experiments that the vigorous intake of carbon dioxide during assimilation in bright light proceeded chiefly through the astomatic cuticle. These experiments demand experimental criticism, which must be reserved until the historical survey is complete. It may be here pointed out that his critical experimental proof consisted in showing of two otherwise similar leaves, that the one which had its stomata mechanically blocked did not decompose any less carbon dioxide than the leaf with its stomata in open functionable condition.

Barthélemy (4) published his first paper on this subject in 1868. After premising on the strength of Boussingault's researches that stomata only play a secondary *rôle* in gaseous exchange, and that there exists no fixed relation between number or size of stomata and assimilatory activity, he brings forward for the first time the view that the cuticle plays a definite specific part in gaseous interchange. This view was suggested to him by Graham's (5) work on the osmosis of gases through thin films of caoutchouc combined with Frémy's (6) then accepted determination of the chemical constitution of cuticle, as being very similar to that of caoutchouc. Carbon dioxide is transmitted through caoutchouc with a rapidity far exceeding that of other ordinary gases, and, of course, it is this gas that must make its way with great rapidity to the internal cells of the leaf during assimilation. Cuticle, then, according to Barthélemy, is to be regarded as a membrane specially fitted by its physical colloid properties to transmit carbon dioxide into the interior of the leaf. Nothing is suggested as to the path by which the equally large quantities of oxygen formed are passed out of the leaf.

In support of his theory he adduced experiments on the osmosis of gases through leaves, which yielded him numbers almost the same as those obtained for caoutchouc by Graham. The experiments are, however, absolutely valueless. CO_2, he concludes, enters leaves almost entirely through the cuticle. As for the stomata, he enunciates the view that

they act as valves, allowing gases to pass out when the internal pressure is high.

In 1877 and 1878, Merget contributed three short notes to the French Academy opposing Barthélemy, and experimentally supporting the stomatic *rôle* by ingenious though rather inconclusive diffusion experiments.

The first (7) attempts to follow the path of diffusion into leaves of those vapours whose passage can be traced by their discoloration effects. Of these, mercury vapour is considered to yield the most decisive results, as it is said that it cannot be carried in a water current nor osmose through a colloid membrane. Boussingault (8) and others have shown the strong toxic action of this vapour, and that it particularly attacks protoplasm, producing dark discoloration and death. If a leaf be exposed to mercury vapour, even the deeply situated cells turn dark in a short time. From this ready penetration by a substance that cannot osmose, Merget argues that the vapour must pass through the stomata and penetrate by the intercellular spaces. Also, if a leaf be injected with mercury and pressed between paper sensitised with $AgNO_3$, the blackening of the paper by the vapour only occurs in those parts which bear stomata. The interpretation given is no doubt correct, but it is not logically conclusive, for the very fact that mercury attacks protoplasm shows that it must osmose through a colloid membrane—the cell wall—and so might enter the leaf partly through the cuticle. One fact the author does establish, namely, that diffusion takes place through stomata with equal facility in either direction. Several short controversial papers by Barthélemy and Merget then appeared without much changing their original positions.

No further papers bearing directly on this subject appeared till 1887, when Mangin published two summary notes (9), (10), and then followed them up in 1888 by a detailed paper (11) of careful experiments on diffusion through cuticle.

The fundamental object of this paper is the determination, firstly, of the quantity of gas that might diffuse under

given conditions across the isolated cuticle; and secondly, of the quantity of this gas absorbed or exhaled by the organ to which the cuticle belongs, in full activity. If the former is less than the latter, stomata must come into use, but if the former is the greater, then osmosis may be sufficient to account for all the exchange.

The diffusion experiments are carried on in an ingenious apparatus invented by the author, in which the cuticle under investigation serves as a diaphragm between two chambers, one of which is kept filled with CO_2, while the other is closed, contains some other gas, and is connected with an adjustable manometer, the movements of which record the amount of gas diffusing into or out of the chamber through the diaphragm of cuticle.

The method appears excellent, but the materials used seem to be open to objections. The cuticle of various leaves is isolated, by allowing the leaf to remain macerating for some weeks in a culture of *Bacillus Amylobacter*, which gradually destroys the rest of the tissues. Pieces of cuticle, two to four centims. square, are thus obtained and employed. In order to render them more resistant, they are painted over with a 10 per cent. warm solution of glycerine jelly. Of this it is alleged that it produced no appreciable increase of the resistance of the cuticle to the passage of gases, and that it will serve to block up the stomata and cracks which have hitherto been the great stumbling-block in such diffusion experiments. Surely it must still be objected, that, since it is so permeable, gas will pass much quicker through those spots where there is nothing but a film of glycerine jelly covering a stoma or crack than through the unperforated cuticle. The osmosis numbers obtained one would expect to be much too high. Thus, prepared cuticle gives numbers for the rates of diffusion of equal volumes of different gases agreeing closely with those obtained by Graham for caoutchouc.

Fallacious as may be the absolute numbers, the relations between different cuticles are of interest. The cuticle of the lower surface of the leaf is always found to be more permeable than that of the upper, but never more than five

times, in spite of the fact that the lower is usually riddled with stomata covered only with a film of glycerine jelly, while there are but few of these weak spots on the upper cuticle. This shows that the actual permeability of the cuticle of the lower surface, apart from the stomata, is never as great as five times that of the upper cuticle, a determination that we shall subsequently see to be of importance.

In the second part of his paper, from experiments on the respiration and assimilation of leaves, the author comes to the conclusion that the amount of osmosis that could take place through the cuticle, as calculated from the above data, is quantitatively insufficient to account for the whole of the necessary gas exchange, but may account for a half to two-thirds of it in respiration. During assimilation, however, he holds that the stomata must carry on practically the whole of the exchange as the tension of CO_2 in the air is too small to produce much osmosis through the cuticle.

In a note to a paper on starch formation from sugar and other substances, Boehm, in 1889 (12), mentions that he has performed experiments which go to prove the prepondering *rôle* of stomata in the gas exchange of assimilation. He exposed thick hypostomatic leaves, laid with their lower faces on the surface of water, to bright light in an atmosphere rich in carbon dioxide, and found that starch was only formed in small areas. He concludes that gases diffuse only extremely slowly through the epidermis cells, and the stomatic path of gaseous exchange must be much the more important.

The most recent work on our subject is by Stahl, 1894 (13), who has anticipated myself in publication of one class of experiments that go to prove the almost absolute *rôle* of stomata in assimilatory exchange.

These consist in showing the local absence of starch formation when a limited area of a leaf is coated on its stomatiferous side with some substance that mechanically blocks the stomata. This forms an easy demonstration experiment ; Stahl tried it on numerous leaves, with the uniform result that, while starch was plentifully formed in normal parts of the leaf, no starch appeared in the coated parts after long exposure to light.

The chief part of my own work (20) on this subject was carried out in 1893 and the whole of it before Stahl's paper appeared. At that time the only piece of experimental work, among those given above, that was held to be above suspicion or explanation was that done by Boussingault, and on this elementary teaching, at all events in England, has been hitherto based. Feeling the need for direct determinations of the intake and output of CO_2 by stomatic and astomatic leaf areas respectively, and finding all the recognised methods of estimation insufficient for my purpose, I commenced, keeping specially in view the needs of biological research, to simplify the technique and improve the delicacy of the one which offers the greatest potentiality of accuracy, namely, the volumetric estimation of the CO_2 by standard solutions of baryta and of an acid. After more than a year's work I have succeeded in designing an apparatus of great chemical delicacy and specially adapted to biological work.

Thus by its aid the evolution of CO_2 by a small area of a foliage leaf can be estimated from hour to hour without a break, for any desired time, while for the same area of leaf, the more active absorption of CO_2 in assimilation can be easily determined for such short periods of time as fifteen minutes, and that at the same time separately for the two surfaces of one and the same leaf area. Further, for the purposes of this assimilation, a current of air containing any desired proportion of CO_2, however small, can be supplied continuously to the tissue under investigation. Moreover, strictly comparable experiments can be carried on simultaneously as the apparatus is practically in duplicate throughout.

The actual estimation of the CO_2 is accomplished by the well-known method of absorption by baryta solution and titration with hydrochloric acid. The novelty consists in this, that only a very small quantity of baryta solution (under 15 c.c.) is employed in each experiment, and that after the absorption the whole of this is titrated with acid in the tube in which the absorption has taken place. Further, the burettes containing the standard solutions are always in air-tight connection with this absorption chamber,

and no air beyond the current under investigation is ever admitted to the chamber, except such as has been carefully freed from CO_2. The special arrangements for stirring and emptying, by means of this air, freed from CO_2, and stored under pressure, cannot be entered into here.

The two currents of air passing continuously through the apparatus are generated by two aspirators of a special type, which, worked on the principle of Mariotte's bottle, gave a practically constant outflow in drops, whatever the level of the water within them, and are adapted to work steadily with small rates of flow (50 to 100 c.c. per hour). These currents enter the apparatus, either through an arrangement for removing the CO_2 when working on respiration, or when working on assimilation through one for adding CO_2. Both these are so constructed that the current never has to bubble through a layer of liquid, and so is supplied to the plant at strictly atmospheric pressure, thus avoiding any risk of drawing gases mechanically out of the part under investigation. The remover of CO_2 is a "tower" full of beads, over which a stream of strong potash flows continuously, and through which the air is drawn. The CO_2 generator is constructed on a new principle, and consists of a tall tube containing fragments of marble, through which the air current passes at a constant rate, while very dilute HCl trickles down it at an extremely slow constant rate. Thus a constant amount of CO_2 is being continually generated and is carried off by the air current. The amount of CO_2 formed can be controlled by the strength of the acid employed. When generating amounts below 2 per cent. of the air current, this arrangement works very satisfactorily. From the CO_2 generator or remover, as the case may be, the current of air passes to the receivers, in which the parts of the plant under investigation are situated. These receivers are of various forms, according to the material experimented on, but are all constructed on the cardinal principle of making them as small as possible consistent with the well-being of the part, in order that changes in the composition of the gas shall, as soon as possible, be felt by the current which passes thence through narrow tubes to

the absorption chambers. When titrations are being made, and the air current can no longer be allowed to pass through the absorption chambers, it passes through a column of water equal in its resistance to that of the baryta solution in the absorption chamber. This enables the rate of flow to be kept constant between, as well as during, the actual experiments. Numerous other details, such as the special method of refilling the burettes, etc., and above all those small points by which constancy is, as far as possible, attained, many of which have involved weeks of special experiment, cannot be described here.

Simplifications of technique by complication of apparatus has been the guiding principle, and the result is that, although the whole consists of at least eight separate pieces of apparatus, many being further in duplicate, and all connected together by a plexus of tubes, yet the working is so automatically arranged that the operator, beyond reading the burettes and occasionally working a finger bellows, has nothing to do but turn stopcocks.

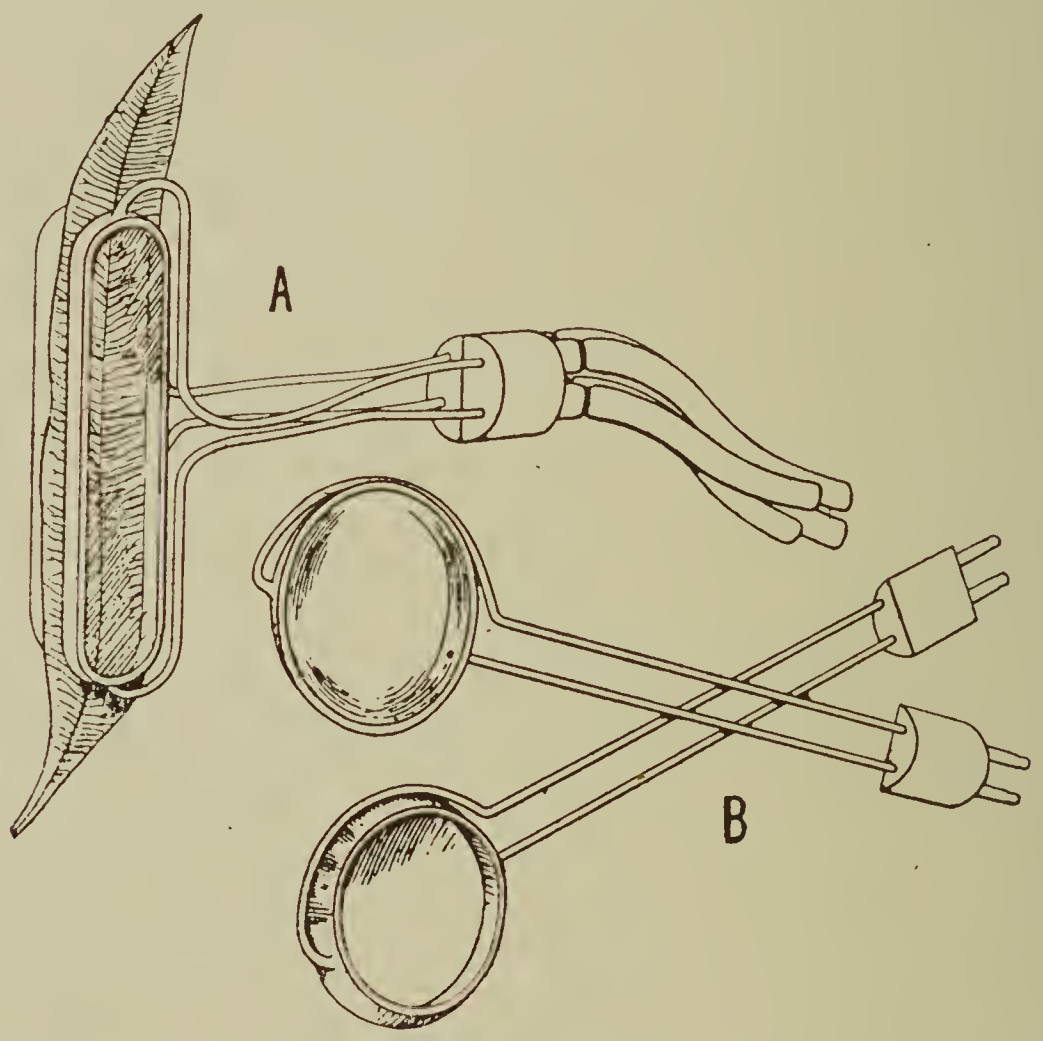
If only one series of estimations is being made, these can be kept absolutely consecutive, the current being led through one of the absorption chambers, while the solution in the other one is being titrated and renewed, and so on alternately. When two series of comparative estimations are being made at once, a small interval must be allowed after each double estimation, during which the titrations are performed; the currents of air in connection with the plants then pass through by-paths, still at their previous rate.

Delicacy of estimation sufficient for present work is obtained by the use of half-decinormal, N/20, standard solutions. Phenolphthalein is used as indicator, and specially delicate end-reactions can be obtained, since atmospheric CO_2 is excluded, and moreover the burettes containing *both* the solutions can be drawn upon.

Series of control titrations often give an error of observation not exceeding 1/200 c.c. of CO_2, and 1/50 c.c. is sufficient for a trustworthy estimation in experiments of short duration.

ON THE PATHS OF EXIT OF CARBONIC ACID FROM LEAVES.

Aided by this apparatus the author has been able to carry out direct experiments on the two surfaces of living leaves, and to determine what relation there is between the distribution of the stomata and the amounts of CO_2 exhaled in the dark or absorbed in bright light. In order to differentiate between the two surfaces, the chambers figured below have been designed. Two different pairs are represented.



Each consists of a brass rim permanently closed on the one side by a glass plate, and attached hermetically on the other side to the surface of the leaf by a little wax, which manipulation can easily be accomplished with the aid of a hot wire. The rim is pierced by two fine metal tubes which also pass through hemi-cylindrical blocks of brass by which the chambers can be clamped together in their right positions on the two surfaces of a leaf.

Two continuous slow currents of air generated by the aspirators are then kept up, through these chambers, over either surface of the leaf. In experiments on respiratory exhalation, the current is freed from CO_2 by the potash "tower" before it enters the chamber. Leaving the

chamber, it passes to the apparatus where the CO_2 it has taken up is absorbed by the standardised baryta. At the end of the experiment, without interrupting the current, the amount of baryta unneutralised is estimated with the standard acid, and thence the amount of CO_2 liberated during that experiment easily calculated.

The area of leaf covered by each capsule is just ten sq. cms., and as the amount of CO_2 given out by surfaces poor in stomata is very small, the experiments on this point are often of twelve hours' duration.

In the appended table of results the amounts of CO_2 evolved, given in the third and fourth columns, are expressed in cub. centims. per hour and ten sq. cms. It is obvious that there is a close general agreement between the distribution of stomata and the exhalation of CO_2.

Plant.	Stomatic Ratio for the two Surfaces.	Upper Surface.	Lower Surface.	Ratio of Amounts of CO_2.
Nerium oleander .	$\frac{0}{81 \cdot 81}$	0·002	0·078	$\frac{3}{100}$
Hedera helix . .	$\frac{0}{81 \cdot 81}$	0·002	0·054	$\frac{4}{100}$
Platanus occidentalis .	$\frac{0}{81 \cdot 81}$	0·001	0·050	$\frac{2}{100}$
Ampelopsis hederacea	$\frac{0}{81 \cdot 81}$	0·003	0·100	$\frac{3}{100}$
Alisma plantago .	$\frac{135}{100}$	0·030	0·025	$\frac{120}{100}$
Iris germanica . .	$\frac{100}{100}$	0·029	0·026	$\frac{110}{100}$
Ricinus communis leaf . . .	$\frac{40}{100}$	0·015	0·036	$\frac{40}{100}$
Ricinus communis cotyledon . .	$\frac{70}{100}$	0·012	0·022	$\frac{55}{100}$
Populus nigra . .	$\frac{18}{100}$	0·010	0·037	$\frac{26}{100}$

The very small amount estimated for the upper surfaces of those leaves which are said to have no stomata above, must be partly made up of the CO_2 passing through the occasional isolated stomata which are known to occur in such cases, and partly of the minute amount of CO_2 which

inevitably leaks or diffuses in from the external air during an experiment of twelve hours' duration.

Allowing for these increments, it may well be that through an absolutely uninterrupted cuticle, no CO_2 at all diffuses if there be an open alternative route through the stomata. We are certainly justified in concluding that the stomata are *practically* the sole path for exhalation of respiratory CO_2.

ON THE PATH BY WHICH CARBON DIOXIDE PASSES INTO LEAVES DURING ASSIMILATION.

The experiments on this point are conducted on essentially the same plan as those on respiration, with the difference that the air coming into the capsules is not freed from CO_2, but is loaded with a definite percentage of this gas by the "constant CO_2 generator".

After the leaf has been carefully fitted up in the capsules figured above with the inlet tubes for each side attached to the CO_2 generator and the outlet tubes to the apparatus, the aspirators are allowed to run for a couple of preliminary hours that the strength of CO_2 may attain constancy. During this time the leaf is kept completely darkened, but must be so arranged that when the covering is removed a bright light falls on the capsules at right angles to the glass side so as to fully illuminate the leaf within. At the end of this time an estimation of the CO_2 passing over either surface is taken for say fifteen minutes, and then the leaf may be illuminated. After a certain length of illumination, a new estimation of the CO_2 is made for the same length of time as before. If any CO_2 has been taken up by either surface of the leaf, the current from that chamber will show a falling-off in its CO_2 content; and, if it does not show this, then no CO_2 has been taken up. Subsequently, the illumination may be altered or stopped, and, after a short time, another estimation made, and so on, ending with a pair of estimations in darkness, when, if all has gone well, the original amounts of CO_2 should be again obtained.

The details of two experiments are appended; the first with a leaf of *Alisma Plantago* which has rather more stomata above than below.

No.	Conditions of Illumination.	Baryta solution neutralised.		CO_2 absorbed by the leaf.	
		Upper surface.	Lower surface.	Upper surface.	Lower surface.
		cub. centim.	cub. centim.	cub. centim.	cub. centim.
1	Dark, 11·45 to 2·5 Estimation, 1·50 to 2·5 . . .	0·30	0·28	0·00	0·00
2	Sun through ground glass, 2·5 to 3·5 Estimation, 2·20 to 2·35 . . .	0·09	0·17	0·10	0·06
3	Sun, as above. Estimation, 2·50 to 3·5	0·00	0·07	0·15	0·11
4	Sun through thin paper, 3·5 to 3·35 Estimation, 3·20 to 3·35 . . .	0·10	0·14	0·10	0·07
5	Shaded on the upper surface; sun on the lower surface, 3·35 to 4·5 Estimation, 3·50 to 4·5 . . .	0·15	0·14	0·07	0·07
6	Dark. Estimation, 5·5 to 5·20 . .	0·28	0·28	0·00	0·00

The baryta solution employed is of such a strength as to unite with just half its volume of CO_2. By this the amounts of CO_2 absorbed by the two surfaces of the leaf, as given in the fifth and sixth columns, are calculated from the falling-off in the amounts of baryta that the CO_2 left unabsorbed is able to neutralise.

The assimilation in this experiment is very vigorous; in reading three, the whole of the CO_2 supplied to the upper surface [0·6 per cent. of the air current] is taken up. The intake is nearly equal on both surfaces of the leaf.

Contrast with this a second experiment made on a leaf of *Ampelopsis hederacea* which bears all its stomata on the lower surface.

No.	Conditions of Illumination.	Baryta solution neutralised.		CO_2 absorbed by the leaf.	
		Upper surface.	Lower surface.	Upper surface.	Lower surface.
		cub. centim.	cub. centim.	cub. centim.	cub. centim.
1	Dark from 2·30 to 5·19 Estimation, 5·4 to 5·19 . . .	0·74	0·76	0·co	0·00
2	Diffuse light, 5·19 to 6·5 Estimation, 5·50 to 6·5 . . .	0·74	0·68	0·00	0·04
3	Sunlight (feeble), 6·5 to 6·32 Estimation, 6·17 to 6·32 . . .	0·74	0·48	0·00	0·14
4	Dark. Estimation, 7·34 to 7·49 . .	0·76	0·78	0·00	0·00

In this case even in direct sunlight no CO_2 is absorbed by the upper surface of the leaf; all that is taken up enters on the lower surface.

From the combined consideration of the concordant results obtained on respiration and assimilation, we see definitely that, at least, as far as carbon dioxide is concerned, gaseous exchange closely follows stomatic distribution on the two surfaces of leaves. From this it results that this exchange takes place practically entirely through the openings of the stomata into the intercellular spaces. The only alternative hypothetical possibility, that in every case the exchange takes place across the epidermis cells and cuticle of those *areas which bear stomata*, and yet not through the stomatal openings, can hardly be seriously entertained. This supposition would involve, among others, the assumption that the cuticle on the lower surface of hypostomatiferous leaves is often fifty to a hundred times more permeable than that on the upper surface. Mangin has however shown that it is never more than four or five times as permeable. The normal difference in gaseous exchange, which is almost of a higher order of magnitude, cannot therefore possibly be explained in this way.

It is further of interest to note the assimilation by the leaf in bright light of its own CO_2 formed by respiration. An experiment was made with a leaf of *Acer platanoides* (all the stomata below) by attaching a capsule to the lower surface only, and supplying to it a current of air freed from CO_2.

No.	Conditions of Illumination.	Total CO_2 escaping.	CO_2 per hour.
		cub. centim.	cub. centim.
1	Direct sunshine, 11·6 to 3·6		
	Estimation, 12·6 to 3·6 . . .	0·00	0·00
2	Dark, 3·6 P.M. to 5·35 P.M.		
	Estimation, 3·55 to 5·35. . .	0·135	0·08
3	Diffuse light, 5·35 to 8·1		
	Estimation, 5·46 to 8·1 . . .	0·04	0·018

The third estimation was however commenced rather too soon after the period of darkness for this to be quite conclusive evidence of escape of CO_2 in diffuse light.

In the second reading we have the CO_2 of respiration made evident, but this in the first reading is wholly assimilated.

This and other similar estimations show conclusively that the classical experiment by Garreau (14) demonstrating the liberation of carbon dioxide in bright light is misleading. That a little CO_2 continually escapes from an entire branch put in a closed chamber with baryta water in the sun is not to be wondered at. The shaded parts of assimilating tissues, together with the non-assimilating tissues of petioles, stems, buds, etc., might quite account for this, but it is difficult to see how any carbonic acid should escape from a completely illuminated leaf surface of normal assimilatory activity. The result of Garreau's experiment is then only an expression of the uncritical conditions in which it was made. Corenwinder (15) showed in 1878 that with young but expanded leaves the chlorophyll of which was not yet fully developed, an evolution of carbon dioxide takes place in the sun, and that this is clearly due merely to the imperfect development of the assimilatory function, for the amount of the CO_2 liberated increases in the dark. With mature leaves he found that no liberation of CO_2 takes place in sunshine.

ON THE GASEOUS EXCHANGE IN LEAVES WHEN THE STOMATA ARE OCCLUDED.

As I have stated above Boussingault has been generally held to have proved that during assimilation CO_2 is taken up chiefly by the epidermis cells of the upper leaf surface which directly adjoin the assimilating palisade cells. He arrived at this conclusion by taking two similar leaves of Oleander, coating the upper surface of one and the under surface of the other with lard, and then exposing them to light in vessels containing known amounts of CO_2 and air. He found that whereas the one assimilated 10 c.c. of CO_2 the other with its stomata blocked assimilated 17·5 c.c. He drew the obvious conclusion that the upper stomata-less surface is the chief seat of absorption and that it is the coating of this with lard that reduces the assimilation to 10 c.c. of CO_2 in the first case.

For some time after I proved, by the direct experiment described in the previous section, that the absorption does not take place through the astomatic surface, this result of his was an enigma to me.

That the cuticle of leaves was slightly permeable to CO_2 was well known, but how was one to explain the greater intake of CO_2 in the leaf with its stomata blocked than in the leaf with its stomata open? It occurred to me that the explanation might lie in the large CO_2 content of the gas in which Boussingault exposed the leaves. The CO_2 formed more than 30 per cent. of the mixture, and the less decomposition of CO_2 in the leaf with stomata open might be due to its receiving not *less* CO_2 than the other leaf, but *more*, indeed so much more above the optimum for assimilation that it decomposed much less than the leaf inside which the CO_2 was not so abundant. Within the latter, CO_2 would never become very plentiful, diffusing slowly as it must through the upper surface of the leaf, as the stomata were blocked with lard. I then turned to Godlewski's well-known paper (16) on the optimum strength of CO_2 for assimilation, and found that he had worked on this same leaf, *Nerium*, and that although his results are not very decisive, there was evidence that the Boussingault result could be interpreted in this way. Thus Godlewski found the optimum strength of CO_2 for assimilation in *Nerium* to be decidedly lower than the 8 per cent. which is the number for other leaves. On looking through his experiments with *Nerium* there will be found one, No. xxxiv., which just seems to fit the case in question. In this experiment he exposed two leaves to the light in 28 per cent. CO_2 and 3·6 per cent. CO_2 respectively, and the amounts of CO_2 decomposed were as 10 : 17·8. Assume that from an environment of 30 per cent. CO_2, the CO_2 diffuses through the cuticle of the leaf with blocked stomata at such a slow rate that there is only as much CO_2 inside the leaf as would result from about 3·6 per cent. outside were the stomata open, and the whole contradiction is gone.

The truth of the above explanation of Boussingault's result will be confirmed if it be shown that in an atmosphere which contains an amount of CO_2 that is but little, if at all,

above the optimum for assimilation, the reverse result is obtained and the leaf with open stomata decomposes more CO_2 than the one with the stomata blocked. I have, therefore, performed a series of experiments similar to Boussingault's, but with leaves of *Nerium* in varying percentages of CO_2, and have found that in a high percentage the leaf with its stomata blocked always decomposes more than the normal leaf, while in low percentages it decomposes less.

These experiments were all performed, with slight modification, by the method brought to perfection by Pfeffer, and described in detail with all its possible sources of error in his paper, "Die Wirkung farbigen Lichtes auf die Zersetzung der Kohlensäure in Pflanzen" (17).

I append a synopsis of the experiments arranged according to the strengths of CO_2 in which they were carried out.

While in 6 per cent. CO_2 the leaf with blocked stomata decomposes only one-seventh as much as the normal leaf, in 50 per cent. CO_2 the relation is completely reversed and it decomposes one and a half times as much.

Mean percentage of CO_2 present in each experiment.	Absolute amounts of CO_2 decomposed per sq. cm. of leaf surface.		Relative amounts of CO_2 decomposed by the leaves with blocked stomata, that of the normal leaves being taken as unity.
	Normal leaf.	Vaselined leaf.	
	cub. centim.	cub. centim.	
6	0·07	0·01	0·14
6·3	0·055	0·01	0·20
7·5	0·046	0·017	0·21
14	0·18	0·04	0·37
55	0·049	0·067	1·3
50	0·043	0·069	1·5
97	0·033	0·060	1·8

Boussingault's work then, thus explained, ceases to support the cuticular exchange theory and yields but a confirmation of the view that the stomata offer a far easier entrance for gases into the leaf.

A due consideration of the differences in assimilation between leaves with stomata open and leaves with stomata blocked brings out the relativity of that which we speak of as the *optimum* percentage of CO_2 for assimilation. The

numbers experimentally obtained for this function really express the *external optimum* of carbon dioxide content—a number very convenient to know, but not of direct physiological significance. The *internal optimum* for the atmosphere surrounding the assimilating cells themselves must be a smaller number, which might well be constant for all cases. The external optimal number would then be a combined expression of this constant and a variable, which would depend on the structure of the leaf in question, and would be large for those leaves which, by reason either of the smallest or scarcity of their stomata, the narrowness of their intercellular spaces or the thickness of the leaf, offered considerable resistance to the diffusion of carbon dioxide.

How variable the size and numbers of stomata may be, the determinations given by Weiss (18) plentifully exhibit, and Unger (19) has shown that the volume of the intercellular spaces in leaves may vary from 3 per cent. to 71 per cent. of the total volume of the leaf. Whether all this variation really does produce a marked effect on the penetrability of leaves we cannot yet say.

As a corollary to the experiments just given, mention must be made of the effect of exposing leaves that have their stomata blocked with vaseline to so small a percentage of carbon dioxide as that occurring in the atmosphere. Here the difference between the vaselined leaf and the normal leaf is very marked, and, according to the usual phraseology, the vaselined leaf does not assimilate at all, that is to say, it forms no starch on exposure to bright light.

If a leaf with its stomata all on the lower surface be freed from starch by darkness, and then, with half the under surface coated with vaseline, be exposed for a whole day in a good light, no starch will be found in this half, while the other has formed starch freely. Coating the upper surface has no effect on starch formation. I performed such experiments in the summer of 1893 and showed them to several botanists. Nearly at the close of my research, in July, 1894, Stahl (13) published similar experiments. He found a mixture of cocoa-butter and bees-wax preferable to vaseline. His observations are much more extended than mine, and he gives some interesting figures of the starch

formation that takes place round wounds or openings in the upper epidermis under these circumstances. I obtained the best results with leaves of *Vitis*, *Polygonum sacchalinense*, and especially with *Sparmannia*. In its suitability for demonstration purposes this experiment should be of great service in dispelling the established English view as to the paths of gaseous exchange.

In conclusion it must be noted that throughout only the carbon dioxide of the gaseous exchange and not the oxygen has been localised. Chemical analysis hardly permits us to attempt any such work with this latter gas. It can, however, hardly be doubted that its movement is normally limited to the stomata as much as that of carbon dioxide. For, on the one hand, it must theoretically diffuse more readily through small openings such as stomata, since its density is less, and, on the other hand, it has been experimentally proved to pass through cuticle less readily than carbon dioxide.

The passage of CO_2 into the leaf during assimilation is the most difficult and crucial of the four movements of gases, two of respiration and two of assimilation, that take place. In any of the other three the differences of tension between the inside and outside of the leaf may increase to almost any extent, while for this case the difference of tension never exceeds 0·3 millim. Hg., and the cuticle of land plants is practically impenetrable to this small tension.

The internal structure of leaves is generally in accordance with the view that all gaseous exchange takes place through stomata, for the intercellular spaces narrow towards the palisade cells, forming a sort of funnel with the wide end at the stomatal region. The active cause of the divorce which results in the palisade cells and stomata, whose united action is necessary for vigorous assimilation, being usually found on opposite surfaces of the leaf, is presumably the double necessity for at once limiting transpiration and illuminating the assimilating cells.

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THE ZOOLOGICAL POSITION OF THE TRILOBITES.

THE veil of mystery which has so long hung over these early inhabitants of the earth has, within the last few months, been almost completely withdrawn. The apparently endless variations of the Trilobites, none departing far from the common type, have so long excited the admiration of intelligent students of nature, and have given rise to so many speculations as to their relationships, that an account of the most recent discoveries can hardly fail to be welcome to a wide circle of readers.

The fact that only the dorsal surfaces of the Trilobites have been, as a rule, preserved, has always been especially tantalising. While few natural objects have been so closely and exhaustively studied as the jointed dorsal shields of the Trilobites, their under surfaces have hitherto eluded even the most searching investigations. Beyond the great labrum, very little could be found which with any certainty could be ascribed to the under surface of the original animal. It is this long-looked-for under surface which has now been partially discovered in at least two species.

Although the new light has burst thus suddenly upon us, the patient researches of naturalists, like Henry Woodward and Walcott in recent times, not to mention the earlier workers, have not gone altogether unrewarded. Fragmentary glimpses of limbs, parts of limbs, and even parts of series of limbs have been brought to light. But so dense has been the mystery enveloping the group, so easy was it to find traces which might be limbs or might not, that all "discoveries" of this kind were looked upon with suspicion.

The limbs have now at last been discovered in a way which leaves little to be desired, and it is now possible to assign to the Trilobites their real position in the scale of animate nature with an accuracy for which, two years back, no one would have dared to hope. It must not, however,

for a moment be supposed that we know all about the Trilobites—far from it. In spite of the new and splendid discoveries in America, our knowledge remains very fragmentary. But among the additional facts which are now in our possession are just those which were most wanted by the morphologist to enable him to give the Trilobites their true place in the animal kingdom.

As I propose to pass these new facts in review and to discuss their bearings, a longer retrospect must be dispensed with. Suffice it to say that until comparatively recently zoologists, relying upon comparisons with existing groups, were generally agreed that the Trilobites were in some way related to the existing Crustaceans, but as to the nature of the affinity opinions greatly differed. In 1881, however, Professor Lankester[1] connected them with the Arachnids rather than with the Crustacea, basing his argument upon an elaborate comparison of *Limulus* and certain *Eurypterids* (both of which are undoubtedly related to the Trilobites) with *Scorpio*. This suggestion is still a matter of controversy, and a review of the arguments for and against would take us too far from our present subject. Further, the Trilobites, in spite of their prime importance in all discussions as to the affinity of the Merostomata, play only a passive part in the discussion, being dragged in, as if of small account, by their specialised offshoot *Limulus*. We may, however, at once state that the new evidence as to the position of the Trilobites is entirely in favour of the older view which linked them with the Crustacea.

Returning then to this older hypothesis, two views were sufficiently clearly defined to admit of discussion and criticism. One sought to ally the Trilobites with the Phyllopods, and relied chiefly upon the variability in the segmentation common to these two, and also upon the likeness between the head shield of *Apus* and of other extinct Phyllopods with the head shield of the Trilobites. The other saw in the Trilobites the ancestors of the modern Isopods, and relied chiefly upon the great external resemblance existing between the two forms. Neither case was really very strong. The absence in the

[1] "*Limulus* an Arachnid," *Q. J. M. S.*, vol. xxi.

Phyllopods of anything like the pleuræ of the Trilobites; and the difference between the leaf-like limbs of the former and the filamentous limbs attributed to the latter, were acknowledged difficulties against the former supposition. While on the other hand, the fixed segmentation of the Isopods and their uniform division into well-defined head, thoracic, and abdominal regions showed them to be far more highly organised than the Trilobites. Lastly, against both these affinities the total absence of any traces of antennæ in the Trilobites served as a warning against any too rash assertion of their Crustacean affinities.

So far, however, as this absence of the antennæ in the Trilobites could tell us anything, the advocates of the Phyllopodan affinity were better off than the advocates of the Isopodan affinity. In the Phyllopod *Apus*, the antennæ are not visible from the dorsal surface, being ventrally placed. If the antennæ in the Trilobites were like those of *Apus*, they would have vanished with the rest of the ventral appendages. Their absence in the Trilobites therefore is just what their assumed relationship with *Apus* would lead us to expect. On the other hand, the same fact was a serious difficulty against the Isopod relationship, as these have very pronounced, anteriorly-placed antennæ, which made it difficult to understand how they could be completely absent in the Trilobites, if there were any genetic relationship between them.

In 1892 the present writer made a detailed study of the morphology of the Phyllopod *Apus*, which led him to the conclusion that it was the most primitive of all existing Crustacea, and was, indeed, an ideal transition form between the chaetopod Annelids and the higher Crustacea. Such a conclusion necessarily led to a comparison between *Apus* and the earliest known fossils which had any claim to be considered Crustacea, *viz.*, the Trilobites. Several new points of resemblance were discovered, of which one was of prime morphological importance. The most startling feature in the organisation of *Apus* was shown to occur also in the Trilobites.

The peculiarity referred to is the remarkable character

of the system of appendages in *Apus*. They commence anteriorly, one well developed pair on each segment. But from before backward they progressively diminish in size, and about half-way along the trunk commence to be more and more crowded. The most posterior pairs are quite microscopical, and so crowded are they that as many as six pairs appear to occur on one and the same body segment. There are occasionally as many as sixty to sixty-five pairs of appendages in all! This phenomenon is quite unique among existing Arthropods, and had long appeared inexplicable. The explanation offered by the present writer was briefly as follows :—

Apus, as the most primitive of all extant Crustaceans, had for its immediate ancestors animals with some sixty to seventy developed segments. In *Apus* these sixty or more inherited segments commence to appear, but the bulk of them remain rudimentary. We thus have the most developed limbs anteriorly, while posteriorly they decrease in size until they are little more than closely crowded limb-buds. Each pair of limbs, therefore, represents a true segment. In the posterior trunk region of *Apus*, where more than one pair of limbs occurs on a body ring, this latter is not a true segment, but a secondarily formed segment, consisting morphologically of as many true segments as there are pairs of limbs developing from it.

This explanation was strongly confirmed by an examination of the internal organisation of *Apus*, in which the progressive rudimentation from front to back which it postulated could be clearly traced, notably in the arrangement of the musculature in the heart, and in the genital glands. On the other hand, that each limb-bud represented a true segment seemed to be established by the presence of a distinct ventral ganglion for each pair. Lastly, an appeal to the developmental history left little doubt as to the correctness of our interpretation of this long-standing enigma. Allowing for special modifications of the anterior functional appendages of the *Nauplius*, it is, at any stage, merely a small *Apus* showing the same phenomenon. The anterior functional limbs are followed by a series of

gradually diminishing rudimentary appendages. The fully developed *Apus* is, in fact, little else than an enlarged, not metamorphosed, *Apus Nauplius*. At any stage at which the growth stopped there would always be a number of progressively diminishing limb-buds and undeveloped segments. We accordingly find in the different species of *Apus* a very varying number of these rudimentary limbs behind a certain number of well-developed appendages at the anterior end of the body.

This argument is still further strengthened when the character of the limbs is also taken into account. A distinct tendency towards the formation of jointed filamentous appendages can be seen at the anterior end of the trunk. This is gradually lost towards the posterior end, and the limbs become more and more leaf-like and primitive, *i.e.*, they are flat unjointed skin-folds.

Here then was an immense amount of evidence to show that the immediate ancestors of *Apus* must have had upwards of sixty well-developed segments. *Apus* then must stand at the very bottom of the genealogical tree of the modern Crustacea, with their nearly constant number of segments, twenty to twenty-one. Indeed, as above stated, so low did *Apus* appear in the scale of Crustacean organisation that it seemed to form a true connecting link between the Crustacea and the Annelids.

Now this same remarkable character in the segmentation, its fixation at the posterior end of the body in a rudimentary condition, which is unknown in any other living Crustacean besides *Apus*, is visible in many Trilobites. The segments taper away at the end of the body, decreasing progressively in size. So feebly developed, indeed, are the most posterior segments, that I am disposed to consider the pygidium of the Trilobites as a modification largely due to the difficulty of bending such rudimentary segments one upon the other. The earliest known genus, *Olenellus*, has the diminishing segments free to the end, and is, in this respect, even more primitive than *Apus*. The free condition of the segments in *Olenellus* shows that the pygidium,

which sometimes exhibits no traces of segmentation, was in fact a secondary modification.

But this was not all; in addition to the visible posterior tapering away of the segments, Walcott's[1] hypothetical restoration of the under surface, based upon his own and previous discoveries, showed the limbs diminishing in size towards the posterior end of the body, as they do in *Apus*. There was then no escape from the conclusion that the Trilobites were, like *Apus*, derivatives of forms with still richer segmentation than they themselves possessed, and that they, like *Apus*, were shortening their bodies by the fixation of a number of the posterior segments in a rudimentary or larval condition. When, therefore, we added to this the striking resemblances between their head shields, the similarity in form and position of the large labrum, the presence in both of an intestinal tube bent sharply round anteriorly and ventrally so as to open backwards above the labrum, the association of *Apus* with the Trilobites ceased to be a matter of speculation, and it was safe to predict that when actual discovery revealed to us more details as to Trilobite structure, organs would be found homologous with, and probably closely resembling, those of *Apus*. For instance, a pair of minute cirrus-like antennæ could safely be assumed to have existed ventrally on each side of the labrum of the Trilobites, and thus not visible from the dorsal surface.

There was only one objection which might perhaps have been raised against this association of *Apus* with the Trilobites, though hardly by a morphologist. The limbs of *Apus* are phyllopodan, *i.e.*, flat, leaf-like, swimming appendages, while those of the Trilobites, as far as the evidence went, were filamentous, and, according to Walcott's restoration, filamentous even in the rudimentary pygidial segments. This difficulty, by no means insuperable, has, as we shall presently see, been completely disposed of by the recent discoveries.

The first here to be recorded is not one of the

[1] "The Trilobite: New and Old Evidence Relating to its Organisation," *Bull. Mus. Comp. Zool.*, vol. viii., 1880-81.

most recent. I refer to Walcott's discovery of the fossil *Protocaris Marshii*, which shows that *Apus*-like Crustacea were contemporaneous with the very oldest known Trilobites.

Of the new discoveries which have thrown so much light on the details of organisation of the Trilobites, the first was in August, 1893, when Mr. W. D. Matthew announced the discovery of specimens of *Triarthrus Beckii*, showing clear traces of antennæ and of other limbs.[1] The first specimen was discovered by Mr. W. S. Valiant, as long ago as 1884, in the Utica slate, but it was not until 1892 that he found other specimens near Rome, New York, which put the matter beyond question. So fine-grained is the matrix, that not only the limbs, but the setæ on the limbs, have left distinct impressions. This discovery, so long waited for, naturally riveted the attention of those who had studied the subject at all. It is, however, the name of Dr. C. E. Beecher which in the future will be most intimately associated with this advance in our knowledge, owing to his patient unravelling of the finer details of structure and organisation, as these could be gathered from a comparison of many specimens. In a series of papers, he has described, in addition to the antennæ, the limbs of the thorax, the limbs of the pygidium, the limbs of the head, with the metastoma or under lip of *Triarthrus*, and, besides these, some of the posterior limbs of *Trinucleus*, including those of the pygidium.

I propose here to discuss a few of the more important of these new details, and to show how they reveal the true position of the Trilobites in the genealogical tree of the Crustacea.

We owe the discovery of the antennæ and limbs of *Triarthrus* chiefly to the fact that, in that form, these appendages were so well developed as to project all round beyond the edge of the dorsal shield. It is a common phenomenon in the animal kingdom to have one member of a group specialised by the great length of the appendages, all

[1] "On the Antennæ and other Appendages of *Triarthrus Beckii*," *Amer. Journ. Sci.*, ser. 3, vol. xlvi., p. 121.

lengthening alike, the antennæ as well as the legs. The specimens of *Triarthrus*, then, were found with a pair of cirriform antennæ projecting from under the head shield, and a confused fringe of appendages protruding, along each side to the posterior end of the body, from under the dorsal shield. The fact that the antennæ came from *under* the head shield was important, as the antennæ of *Apus* are attached under the head shield on each side of the prostomium or large upper lip, whereas, as a rule, among the higher Crustacea, the antennæ are placed quite anteriorly. And, as a matter of fact, examination of fortunate specimens showed that the antennæ were inserted in *Triarthrus* in practically the same relative position to the prostomium as they are in *Apus*.

If verifications of predictions based upon a theory are proof of the correctness of that theory, here was the proof of the relationship between *Apus* and the Trilobites. That relationship had been based upon other resemblances of organisation, some of the more important of which have been briefly indicated above, and little more than a year after the antennæ of the Trilobites were found exactly where it was predicted they would be found. The importance of this point will be understood when it is remembered that no other known Crustaceans, recent or fossil, except *Apus* and the Trilobites, possess the great square labrum pointing backwards, with the antennæ inserted on each side of it, the whole hidden under a great head shield.

Before passing on to the next point, this ventral position of the antennæ and labrum deserves attention. Occurring in *Apus* with its traces of sixty segments, and in the Trilobites, the oldest fossil Crustaceans, no one can doubt that it is the primitive position of these organs in the crustacean phylum. It follows, therefore, that the annelidan ancestors of the Crustacea, with their originally *anteriorly* placed prostomium, must have bent the first segment sharply round in order to bring that organ into the ventral position found in the primitive Crustacea. There is, in fact, abundance of evidence to show that such a bending did take place. Without going into details which have been

elaborated elsewhere, I may mention a few points, *e.g.*, the sharp anterior bend in the intestinal canal; the curious clumping of the ventral longitudinal muscles, which form the "cartilaginous" endosternites in the angle of the bend; the corresponding stretching out of the longitudinal muscles on the dorsal side; the simple explanation this bending round of head segment affords of the translocation of two pairs of limbs, originally post-oral, into the pre-oral position occupied by the antennæ in the Crustacea; and lastly, the first ontogenetic appearance of the eyes in *Apus* in a more anterior and ventral position than they possess in the adult, and their subsequent wandering upwards on to the dorsal surface. In the discussion of the remaining points which we shall select for mention in the following pages, we shall, therefore, assume that the organisation of these early Crustacea, *Apus* and the Trilobites, can be deduced from that of an Annelid with the first segment bent round ventrally, so that the large prostomium points backwards, while the pair of appendages, specialised into sensory cirri, which were arranged one on each side of it in its original anterior position, remained on each side of it in its new ventral position, and became the first pair of antennæ of the Crustacea. In making this assumption, we are in no sense rushing ahead of our facts; not only does the evidence already brought forward justify us, but the points still to be dealt with confirm it in every instance. We state it thus definitely at this point chiefly in order to facilitate the comprehension of other details.

Having mentioned the large labrum which has long been known, and the antennæ attached at each side of it, the structure next in order, from before backwards, is the under lip, which has recently been described by Dr. Beecher for the first time, as it appears in *Triarthrus*.

The under lip is simply the ventral border of the mouth, but in *Triarthrus* it is bent round as a small ridge projecting backwards. In the original annelidan ancestor, with its anteriorly placed mouth, the dorsal border was formed by the large fleshy labrum or prostomium, while the ventral border, in all probability, showed no specialisation whatever.

As soon, however, as the first segment bent round ventrally, so that the mouth opened backwards, the prostomium would take up the position of the labrum in *Apus* and the Trilobites, and the ventral border would form a small ridge-like under lip, also pointing backwards, such as is actually found in *Triarthrus*. This under lip in *Triarthrus* is thus another slight confirmation of our deduction of the primitive Crustacea from an Annelid with its first segment bent round ventrally.

This under lip of *Triarthrus* is however no longer the simple ridge we must assume it to have been in its most primitive condition. It already shows a tendency to form lateral lobes. These small lobes, so insignificant in themselves, become important when studied comparatively. Their remarkable development in *Apus* reveals to us their function. They supply us with an interesting illustration of the dependence of morphology upon physiological need. In order to make this clear we shall have to make a brief digression to explain the new method of feeding which the bending round of the mouth implies.

The chaetopod Annelids, with their anteriorly placed mouths, largely feed by protruding an evaginable, and generally armed, portion of the pharynx. This is thrust out, seizes the prey, and is then withdrawn back again into the mouth. Any new position adopted by the mouth could only be in adaptation to some new method of feeding. We have, therefore, to see what advantages would be afforded by the bending round of the mouth ventrally backwards. There is an immense amount of nutrient matter, animal and vegetable, which, coating the rocks and weeds, is quite unattainable by the shooting out of a proboscis. Any Chaetopod, therefore, learning to browse in such a way as to be able to take advantage of this hitherto unattainable supply of food, would stand a good chance in the race for life. I have, for instance, long thought that the great phylum of the Mollusca might be deduced, in a manner which I think can be worked out in detail, from Chaetopods which early learnt to feed by scraping the surfaces of things with the ventral edge of the protrusible

pharynx, this, in time, becoming specialised as a radula. And so I believe that the great phylum of the Crustacea arose from Chaetopods also becoming adapted to take advantage of the same food supply, but in a manner very different from, and less efficient than, that adopted by the Mollusca. The ancestor of the crustacean phylum used its appendages as the instruments for obtaining supplies. The food raked from each side by the parapodia into the middle line, was then pushed forward towards the mouth, bent round to receive it. This new function of the parapodia, which were primitively protective on account of their bristles, and locomotory as swimming plates, would soon lead to modifications of structure, which we are not yet in a position to describe in detail. The most primitive phyllopodan limbs with which we are acquainted, *viz.*, the larval limbs of *Apus*, represent the earliest known stage in the transformation of the annelidan parapodium into the crustacean legs. They are flat unjointed skin-folds, carrying on their dorsal edges a pair of processes, one of which has strong claims to be the typical gill of an annelidan parapodium. Their ventral edges are deeply notched so as to form a series of lobes. The proximal lobe of the series is distinct from the rest, and can safely be taken to be the original ventral parapodium.[1] We further find in *Apus* that these innermost lobes (the ventral parapodia), forming a longitudinal series along each side of the middle line, are twisted round in such a way as to show that in addition to assisting to rake food together, they also serve for pushing it forward towards the mouth.

Having thus briefly described the new method of feeding of the ancestor of the crustacean phylum we must return to the ridge-like metastoma of *Triarthrus* and its commencing modifications.

It is not difficult to see that, in its most primitive, ridge-like form, such a metastoma would be a barrier across the ventral surface, hindering the pushing forward of food into

[1] I have figured such a limb, *viz.*, the last limb in *Apus productus*, in *The Apodidæ*, fig. 20, p. 48.

the mouth by the appendages. In order to perfect this method of feeding, this ridge would have to be modified. In *Apus* we find, in fact, that it has almost entirely disappeared in the middle line, while its lateral ends have developed into two fleshy projections looking somewhat like jaws, and often mistaken for such. Thus modified, the metastoma, on the one hand, ceases to be a hindrance against the travelling forward of food along the middle line into the mouth, while, on the other hand, its lateral lobes help to prevent the escape of food on either side when the mouth is reached. Now the metastoma of the Trilobite *Triarthrus* is especially interesting because it still shows the primitive ridge right across the ventral surface, not yet smoothed down in the middle line, while the two ends are only beginning to form their lateral lobes. The discovery of the metastoma of *Triarthrus* is thus another confirmation, this time not only of the relationship between *Apus* and the Trilobites, but also of their common derivation from an Annelid modified in the manner described.

Turning now to the limbs, they offer so many striking confirmations of the same conclusions that they alone are, it seems to me, sufficient to placé the relationships here set forth beyond further question. We will briefly note some of the leading points.

(*a*) Our deduction of both *Apus* and the Trilobites from a common annelidan ancestor, from which neither was very far removed, requires that all the appendages except the first pair should have been behind the metastoma. This primitive condition, lost in *Apus* by the metastomial lobes stretching behind the third pair of limbs, is apparently found in *Triarthrus*.

(*b*) The series of appendages behind the metastoma should have been structurally alike, that is, there was no original specialisation of those limbs nearest the mouth into mandibles and maxillæ as distinct from locomotory appendages. This primitive condition is retained by *Triarthrus*.

(*c*) While the first pair of appendages is reduced to a pair of cirri, one on each side of the labrum, the pairs

immediately following are, in the Chaetopods, often reduced in size, progressively increasing till a maximum is reached. This primitive annelidan characteristic is found in *Triarthrus*, as well as in the related *Limulus* and *Eurypterus*; the sixth pair in the last two animals, as well as in *Apus*, being the most powerfully developed.

(*d*) Their hypothetical annelidan ancestor requires that both the Trilobites and *Apus* originally possessed primitive phyllopodan appendages. This has now been fully established by Dr. Beecher's discovery that the rudimentary limbs under the pygidium of two Trilobites, *Triarthrus* and *Trinucleus*, were phyllopodan, like the larval and rudimentary limbs of *Apus*.

(*e*) Such phyllopodan limbs must have extended along the whole length of the body as far as the metastoma. In *Apus* the phyllopodan type persists as far forward as the sixth segment, and can be faintly traced as far as the third; while *Triarthrus* teaches us, as we have seen (*b*), that the homogeneity of the limbs extended originally as far forward as the second pair. We have thus a conclusive proof that the common ancestor of both *Apus* and the Trilobites possessed simple phyllopodan appendages along the whole length of the body from the second segment.

These are some of the new points relating to the appendages as a whole, which arise out of a comparison of Dr. Beecher's discoveries with the known structure of *Apus*. There are many other confirmatory points of interest and importance which, if space allowed, might be adduced, but these are the ones which I have selected to emphasise, because they demonstrate once for all the affinities of these early Crustaceans to one another as common derivatives of an annelidan ancestor, from which neither is far removed.

Having thus briefly traced the Trilobites and their existing ally *Apus* to their common ancestral form, it remains for us to indicate, as far as our knowledge goes, their respective divergent specialisations. However interesting it is to run them back to their common form, it is perhaps even more interesting to trace the lines of advance leading them to their different

fates—the Trilobites to die out with a single survivor in *Limulus*,[1] *Apus* not only to survive to this day, but to give rise to the whole family of modern Crustacea.

Before describing the lines along which the two forms must have diverged, one other very early specialisation of the common racial form has to be mentioned. The bending round of the prostomium with the antennæ, and of the parapodia with their protective bristles, towards the ventral surface, left the dorsal surface quite exposed; some compensatory arrangement for protection was therefore needed. It is not improbable that the dorsal organ, which in the larva is comparatively of great size, and forms a shield-like plate of excretory cells, was protective. In addition to this, however, the skin of the dorsal surface at a very early stage not only thickened, but grew out laterally into folds which roofed the animal over, while the skin of the under surface retained its primitive delicacy and softness, hence its almost universal absence in the fossil Trilobites. The earliest arrangement of these dorsal shields must for the present remain matter of conjecture. The very earliest stage, both ontogenetic and phylogenetic, known to us is that presented by the minute larva of *Olenellus*, the most primitive Trilobite. We there find an enormous skin-fold forming a crescent round the front and sides of the first segment, the three following segments having much smaller lateral folds (pleuræ), which, fusing with this crescent fold of the first segment, together form a roof almost completely protecting the young *Olenellus*.[2] There is some reason to believe that primitively this dorsal roof was confined to the anterior end of the body covering the first five segments. From this common stage the Trilobites, and the Crustacea proper, parted company.

[1] I am inclined to believe that the *Ostracoda* also may be deduced from larval Trilobites in which the head shield folded longitudinally to form the bivalve shell (*cf.* *The Apodidæ*, p. 256).

[2] From such extremely primitive crustacean or rather pre-crustacean larvæ with their great circular roofs, I would deduce the *Ostracoda*, and the enigmatical palæozoic *Cyclus*.

The Trilobites seem to have perfected the browsing manner of life, spending the whole of their active life in crawling over the sea bottom. The primitive dorsal shield at the anterior end of the body repeated itself on every subsequently developed segment in the form of a pair of sharp blade-like pleuræ. The Trilobites thus browsed securely under a great jointed roof, each joint being provided with a pair of formidable lateral spines. The development of these enormous pleuræ may have early tended to limit the number of segments. In adaptation to this settled creeping manner of life, the phyllopodan limbs became early transformed into filamentous walking legs. The limbs, commencing from the second pair, were used as locomotory (walking) appendages (*cf. Triarthrus*, *Limulus*). Only under the pygidium, in the rudimentary segments, did a few of the appendages remain phyllopodan. The function of raking together the food, and pushing it forwards to the mouth, must have been entirely carried on by the ventral branches of the parapodia. In *Limulus*, we find these "gnathobases" highly developed, and carrying a cirrus-like appendage, while in *Triarthrus* (*cf.* Dr. Beecher's figures) those nearest the mouth are plate-like jaws, while those on the trunk segments have a remarkable leg-like appearance which can hardly be accidental. I would like to suggest that some special development of these gnathobases might be expected in the Trilobites, that is, *if* the dorsal leg-portions were purely locomotory.

These are a few of the more important points of interest in the specialisation of the Trilobites. The type was plastic enough to give rise to endless small variations, but apparently not plastic enough to re-adapt itself to certain new, and at present unknown, changes in the conditions of life; hence the extinction of the race.

The other great branch of these primitive annelidan Crustacea has had a very different fate. The head shield was not primitively repeated along the body as pleuræ, but grew backwards as a great fold, forming a carapace which protected the back and sides of the trunk. The trunk segments remained simply cylindrical

and persisted for a time in far greater number than in the Trilobites. The limbs further remained phyllopodan as swimming plates. While, however, thus retaining primitive conditions in the trunk, the limbs of the anterior or head segments underwent a very marked specialisation. The first pair remained as simple antennæ, the second pair degenerated and lost their jaw plates, but ultimately developed their sensory functions to become the second antennæ of the typical Crustacean. The third pair lost their locomotory portions and became the powerful mandibles working between the labrum and labium (or labial lobes). The two following pairs also more or less completely lost their locomotory functions, the basal portions persisting as accessory jaws, the maxillæ. This formula for the head limbs, seen in its most primitive condition in *Apus*, became from henceforth the typical mouth formula of the Crustacea.

From such a form as this, all the modern Crustacea can be deduced.[1] The segmentation became more specialised and consequently less rich, the limbs lost their primitive phyllopodan characters, the two pairs of antennæ travelled forwards to the anterior edge of the head; the mouth and mouth-parts also moved forward, causing the almost complete obliteration of the great labrum. The metastomial lobes persist as the "paragnathes," while the great fleshy mandibles of *Apus* became hard chitinous plates. The degenerate locomotory portions of the maxillæ became the palps, while, in some forms, a certain number of trunk limbs, the ventral branches of which, in the primitive form, served to push food forward towards the mouth, move forward and join the maxillæ, as accessory jaws, the maxillipedes. The great carapace has undergone many variations; in some cases it has degenerated, in others it has given rise once more to a system of pleuræ along the trunk by segmental repetition, and forms such as the Isopods have arisen, closely resembling the Trilobites, and perhaps to some extent taking their place in the modern seas.

[1] *Cf.* on this Prof. K. Grobben, *Sitzungsber. d. K. Akad. Wien*, vol. ci., 1892, pt. i.

From this brief account of the rise of the modern Crustacea from *Apus*, it follows that the Trilobites are, in reality, pre-crustacean. They represent a stage more primitive than *Apus*, *i.e.*, nearer to the original annelidan ancestor of the whole phylum.

While the immediate ancestors of *Apus* and of the Crustacea proper retained the power of swimming freely from place to place, the Trilobites became specialised for a creeping manner of life, apparently perfecting along one line the new method of feeding, which, as above described, was the physiological change leading to the subsequent morphological transformation of the Chaetopod into the Crustacean.

We may therefore describe the Trilobites as specialised links in the chain which connects the Crustacea with their annelidan ancestors.

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H. M. BERNARD.

SOME METASOMATIC CHANGES IN LIMESTONES.

THE origin of dolomitic rocks has engaged the attention of geologists and chemists from an early time. Some of the older chemical theories may be dismissed without discussion as having at most a very limited application, and we may confine ourselves chiefly to those views in which the rocks in question are regarded as ordinary calcareous deposits, owing their magnesian character to accidents in the main posterior to their accumulation as strata.

In their present condition these rocks seem to consist in general of dolomite or of a mixture of dolomite and calcite, with which may be associated siliceous, argillaceous, or other impurities in varying amount. Those examples richest in dolomite are styled dolomite-rocks, or simply "dolomites"; those in which the mineral named is subordinate to calcite are correctly termed dolomitic or magnesian limestones, though the latter name has been applied (as in the Permian of the North of England) to strata consisting essentially of dolomite. Cotta fixed the limit at 23 per cent. of magnesium carbonate, which corresponds to equal molecular proportions of dolomite and calcite. This percentage should be reckoned, as pointed out by Hopkins (1), on the total carbonates, excluding non-calcareous impurities. We must not exclude the possibility of the simple magnesium carbonate, magnesite, also occurring in some of these rocks, but there seems to be little positive information on this point. However this may be, it seems probable that rocks which on analysis are found to consist of calcium and magnesium carbonates in other than equal molecular proportions, are composed of grains each of which has a definite constitution, *viz.*, that of calcite or that of dolomite (or more doubtfully magnesite). All actual investigations tend to the belief that no true chemical isomorphism exists between the three minerals

named, nor indeed between calcium and magnesium compounds in general. Retgers (2), after a full consideration of this question, came to the conclusion that the carbonates of lime and magnesia examined by him fell under three heads ; *viz.*, calcite ($CaCO_3$) with very little or no magnesia, magnesite ($MgCO_3$) with very little or no lime, and the double salt dolomite of perfectly definite constitution ($CaMg_2CO_3$). As a matter of fact, it is not difficult in the less fine-grained magnesian limestones to verify the mixed nature of the rocks by the action of dilute acid, the more soluble calcite being thus separated from the less soluble dolomite.

Between the magnesian and ferrous carbonates, on the other hand, it seems that there is a true chemical isomorphism, just as in the corresponding silicates (both pyroxenes and olivines). Mineral analyses of the rhombohedral carbonates prove the existence of various connecting links between dolomite and pure ankerite ($CaFe_2CO_3$) and probably between magnesite and chalybite ($FeCO_3$). Indeed Bentivoglio (3) goes so far as to maintain that more or less iron is essential to the constitution of dolomite, since in thirty-six analyses of dolomite rhombohedra from various localities, he found ferrous carbonate never absent.

In speculating on the mode of derivation of dolomitic rocks from ordinary limestones various difficulties arise which cannot be said to have been completely surmounted up to the present time, and it is to these especially that we shall turn our inquiries. These difficulties are connected firstly with the source of the large quantities of magnesia which are supposed in most theories of "dolomitisation" to have been introduced from without, and secondly with the chemical processes by which calcium carbonate may conceivably be converted into the double carbonate, dolomite, under natural conditions.

As regards the source of the magnesia, various early writers ascribed to it a volcanic origin. Although it is by no means certain that dolomitisation may not in some cases have been effected in this way, it is manifest that such a hypothesis cannot be of general application. Other geolog-

ists have supposed the magnesia to be introduced in the form of sulphate and chloride from sea-water, and the frequent association of gypsum with dolomite has led to various speculations on a possible reaction between magnesium sulphate and calcium carbonate. The possibility of such a reaction, resulting in the formation of calcium sulphate and calcium-magnesium-carbonate, raises an obvious difficulty, since at ordinary temperatures and pressures the opposite reaction takes place, gypsum and dolomite decomposing one another.

Other writers, again, have inquired whether the magnesia may not have been contained, wholly or partly, in the rock itself from the time of its accumulation ; and there are considerations which seem to leave room for difference of opinion as to the extent to which the magnesia has really been supplied from some definitely external source. Many calcareous organisms at the present day contain a noteworthy amount of magnesia, and in some of the calcareous algæ (Lithothamnion) the ratio $MgCO_3 : CaCO_3$ reaches an average of 10 : 100. Several geologists have drawn attention to the large part that has been played by various calcareous algæ in the formation of what are now extensive masses of dolomitic rocks. Von Wöhrmann, for example, has ascribed the thick formations of limestone and dolomite which are so prominent a feature of the Alpine Trias to the agency of algæ of the families Codiaceæ and Siphoneæ (4).

Now dolomite is much less readily soluble than calcite and distinctly less soluble than aragonite ; and, if we may assume the magnesia of calcareous organisms to exist in the form of dolomite, it follows that any removal of material under ordinary conditions will leave the residue relatively enriched in dolomite. It has been suggested that the dolomitisation of limestones may sometimes have been effected in this way, rather than by an actual replacement of lime by magnesia introduced in some form from without.

This point has recently been taken up by Högbom in an interesting paper (5). He is dissatisfied with the explanation of dolomitic rocks as ordinary limestones in which magnesian solutions have effected a partial substitution of

magnesia in place of lime, and finds more plausible the suggestion that limestones originally containing some magnesia have become enriched in that constituent by the removal of the lime-carbonate. There seems to be some positive evidence of this process going on to some extent. Thus, while in corals from Bermuda the ratio $MgCO_3 : CaCO_3$ is always under 1 : 100, and in shells still less, the ratio in the reef-rock and lagoon-sediment of the same locality is from 1·7 : 100 to 4 : 100, the higher ratios corresponding to the finer sediments. The author lays stress on the finely divided state of the material, allowing it to remain suspended in water and subjected to the "leaching out" action (Auslaugungsprocess) for a long time. In this connection his examination of the Quaternary muds of Sweden is instructive. These deposits came from the melting of the ice, and doubtless derived their calcareous material from the lower Palæozoic limestones. The latter have $MgCO_3 : CaCO_3$, on the average, in the ratio 1 : 100, while in the muds themselves the ratio ranges from 3 : 100 to 100 : 100 (*i.e.*, to equality). Moreover, on comparing analyses from eight localities, extending from the Gefle district to south of Stockholm, it is found that, as we pass southward, that is away from the source of calcareous matter, while the total carbonates fall steadily from 32·2 to 4·5 per cent., the ratio $MgCO_3 : CaCO_3$ increases steadily from 3·7 : 100 to 36 : 100. Högbom cites also the analyses of deep-sea deposits given in the *Challenger* Report, and shows that as the total carbonates diminish from 87·4 to 4·1 per cent., the ratio in question rises from 0·8 : 100 to 105 : 100.

It must be considered doubtful to what extent the process here discussed is applicable to extensive masses of dolomitic rocks. To convert in this way even a Lithothamnion-rock to a dolomite, as defined by Cotta, would require the removal in solution of four-fifths of the carbonate of lime, or about 74 per cent. by weight of the total material. This can be conceived only in the case of such fine deposits as those specially discussed by Högbom. In dolomitised shore-deposits, in which shells are sometimes bodily replaced by pure dolomite, the greater part of the magnesia

must have been introduced from some external source. It is worthy of note, however, that the matrix of a shelly limestone, which was originally in a finely divided state, seems to be much more readily affected than the enclosed shells.

As regards the possible processes by which calcite can be converted to dolomite by magnesian solutions, various experimental investigations have until quite recently afforded but little assistance. Morlot, in 1847, found that magnesium sulphate and calcium carbonate reacted upon one another at a temperature of 200° C. and a pressure of 15 atmospheres. Marignac, with similar conditions, obtained a reaction between magnesium chloride and calcium carbonate. Hoppe-Seyler, in 1875, obtained a mixture of magnesite and dolomite by the action of magnesium chloride or sulphate on calcium bicarbonate, heated in a sealed tube; the action took place at temperatures between 100° and 160°, but not below 100°. The special conditions necessary for success in such experiments as these make their application to the origin of dolomitic rocks in nature a matter of much doubt. Other possible reactions have been suggested; for instance, that part of a limestone may be converted to magnesite by a solution of magnesium bicarbonate, the excess of lime being removed as calcium bicarbonate, and the magnesite subsequently combining with the remaining limestone to produce dolomite.

More recently Klement (6) has given a discussion of the whole subject of the origin of dolomitic strata, and has made experimental researches which throw an interesting light on some of the difficulties already alluded to in the process of dolomitisation. A brief account of the paper in which his results are embodied will therefore be of some value.

After briefly noticing various former attempts to solve the problem, Klement draws attention to certain ascertained facts which have suggested to him the line taken in his experiments. Typical dolomite, he remarks, is very often found in the form of coral-reefs, and the most complete dolomitisation in the case of atolls. Richthofen has noted in the Trias of the Tirol that the atolls are of dolomite-rock, the barrier-reefs of dolomitic limestone. Dupont, in the

Devonian and Carboniferous of the Ardenne, has specially remarked the dolomitisation of the central parts of the reefs. In the emerged coral-island of Metia or Matea, in the Pacific, Dana has pointed out that the interior, *i.e.*, the part containing the lagoon, is of dolomite-rock with up to 38 per cent. of magnesium carbonate, while the corals of the same reef contain only a trace of magnesia, and he inferred the introduction of magnesia into the rock from sea-water, probably concentrated in a closed lagoon, the process taking place therefore at ordinary atmospheric temperatures. These various facts point to some peculiarity in certain limestones rendering them susceptible to dolomitisation, a peculiarity well exhibited in—though not confined to—rocks of coral origin.

Now the important chemical difference between corals and many other calcareous organisms is that the calcium carbonate in the former is not calcite but aragonite, and Klement was accordingly led to experiment on the action of sea-water on aragonite. The results of his experiments may be summarised as follows :—

Powdered aragonite was acted upon by magnesium sulphate in a saturated solution of sea-salt, the action being found to take place at temperatures above 60° C. The amount of magnesium carbonate formed was found to increase with the temperature, but there appeared to be a certain maximum for each temperature. For 91° the maximum was about 42 per cent. of magnesium carbonate, which is not much less than that in pure dolomite (45·7). In dilute solutions of sea-salt the action was found to be much feebler, and it was also feebler when too much of the solution was used, apart from its degree of concentration. In the absence of sodium chloride the action of the magnesium sulphate, even in concentrated solution, was feeble. Magnesium chloride, substituted for the sulphate, was found to be much less active. The sulphate had very little action on calcite, but acted on corals just as on artificial aragonite.

These experiments go to establish that the magnesium sulphate in sea-water will act upon corals or other aragonite organisms in a warm climate, and in places where the salt in the sea can reach a considerable degree of concentration.

It remains to be inquired whether such action results in the formation of dolomite. This is not a very easy point to settle decisively, but Klement came to the conclusion that the product resulting from his experiments was not dolomite but a carbonate of magnesium alone (neutral but hydrated). He was, therefore, compelled to suppose that there is in nature, not only a formation of magnesium carbonate by a process comparable with his, but further, a combination of this product with the intermixed calcium carbonate to form dolomite.

The crux of Klement's theory is evidently in this latter transformation. There seems to be no direct chemical evidence that such a combination is possible under conditions realised in nature, and the micro-structure of dolomitic rocks does not seem to throw much light upon the question. Our author points to the dolomitisation of crinoidal and other organic bodies as evidence that the assumed combination does take place; but this would seem to prove too much, for crinoids are undeniably calcite organisms. If calcite ossicles of crinoids can be converted into dolomite, which seems to be beyond doubt, we may ask why should not the same transformation affect whole bodies of calcite-rocks, and further, why cannot the process be reproduced in the laboratory?

If the process of dolomitisation of a limestone is something quite independent of the accumulation of the rock, it may presumably be brought about at any distance of time after that accumulation. That it is sometimes long subsequent is proved by its evident connection in various cases with joints, faults, and other geological accidents which have affected the solid limestone strata. Further, the process seems in some cases to have been later than recrystallization in the limestone, whether produced by metamorphism or otherwise. This at least may be inferred from the apparent fact that the dolomitisation seems often to have spared the more coarsely crystalline parts of the rock. The Cambrian limestones in New Jersey, recently described by Nason (7), seem to illustrate this. While the general body of these rocks is of magnesian limestone or true dolomite,

the coarsely crystalline modification near the contact with granitic intrusions is in general non-magnesian. It is perhaps more questionable whether such an idea can be applied to the Durness limestones of the Assynt district in Sutherland, which, according to analyses given by Heddle, are in great part perfectly dolomitised, while the Ledbeg marble, which is their metamorphosed equivalent near the large intrusion of Borolan, is much less magnesian. In these and similar groups of limestones, the facts that certain zones or beds are described as dolomitic, and others alternating with them as non-dolomitic, might seem to suggest that the magnesian nature of the former was closely connected with the accumulation of the deposits. Such an argument, however, is by no means conclusive ; we may with equal reason suppose that at certain horizons in the series of deposits there was something in the character of the rocks which predisposed them to the transformation to dolomite. At least the manner in which one and the same bed may sometimes be seen to pass abruptly from a normal limestone to one composed two-thirds of dolomite proves that, in such a case, the magnesian character has been acquired by some secondary change distinctly posterior to deposition.

The well-known concretions of crystalline calcite in some beds of the magnesian limestone of Durham also suggest themselves. Garwood (8) has shown that the ratio $MgCO_3 : CaCO_3$, which in the matrix of these bodies is 66·74 : 100, is in the concretions themselves only 11 : 100. In his view the formation of the concretions is posterior to the dolomitisation, and has been influenced by the amount of magnesia in the several beds, either too high or too low a proportion checking or preventing the development of the structure. The facts, as stated by him, do not, however, seem to be inconsistent with the alternative supposition that the already existing crystalline concretions have resisted the dolomitising process by which the rest of the rock has been transformed.

Leaving these doubtful questions we may pass on to a few remarks on another common metasomatic change, or series of changes, in limestones ; that which results in the

replacement of the carbonate of lime by various iron-compounds. It is a familiar fact that very many valuable bodies of iron-ores occur not, like most metalliferous deposits, in the form of lodes and veins, but in more or less distinct strata conformable with the associated sedimentary rocks. Granting that, with suitable conditions, iron-compounds, both oxides and carbonate, may be directly formed as stratified deposits, it is still certain that many important cases demand a different explanation.

The frequent pseudomorphous replacement of calcite by chalybite was pointed out by Bischof thirty years ago, while Blum traced as a further change the conversion of the ferrous carbonate into ferric oxide or hydrate. The chemistry of these processes has been fully discussed by Kimball (9). The geological importance of such transformations has become more clearly recognised, however, since the investigations of Judd on the iron-ores of Northamptonshire and Rutland (1875), Sorby on the Cleveland ironstones (1879), and other geologists on various ferruginous rocks in this and other countries. The derivation of many deposits of chalybite, hæmatite, limonite, and magnetite by metasomatic processes from ordinary limestones is now placed beyond the possibility of doubt.

The number of rocks to which this explanation has been applied is greatly increased by the literature of recent years. As instances we may allude to Lotti's account of the Tertiary iron-ores of Elba (10) and to the descriptions given by American geologists of numerous examples in the United States. Foerste's contribution on the Clinton (Silurian) iron-ores of the Eastern States presents some points of interest (11). These, like many other stratified iron-ores, are often oolitic; but in specimens from numerous localities the author finds the grains to show no true oolitic structure, but to be simply rolled and water-worn fragments of bryozoans. As a rule the iron-ore occurs as a replacement of the substance of the bryozoans themselves, while the cementing calcite and that which occupied the cells may have suffered the same change, and sometimes the cement has been replaced but the organic fragments preserved as

carbonate of lime. Smyth (12) has shown, however, that true oolitic iron-ores do occur in the Clinton group, for example, at the type-locality. Each grain there consists mainly of concentric coats of iron-oxide with a central nucleus of a rounded quartz-granule; but, on removing the iron-oxide with acid, a transparent siliceous skeleton is left, showing a concentric structure, often with ten or more distinct layers. From this intimate association of silica with the iron-oxide, and from other considerations, Smyth concludes that this and some other iron-ores described were formed directly as such, and not by metasomatism from limestones. Some of the difficulties in the way of this view are discussed in Kimball's paper (9), while Smyth has given in another place his reasons for maintaining his view of the probable origin of these Clinton iron-ores (13). It may be observed that the presence of a siliceous skeleton in oolitic grains now composed of ferric oxide or hydrate is a feature of wide-spread occurrence, and is found in ores undoubtedly representing limestone strata. Further, it is met with in oolitic limestones which have not undergone any ferruginous replacement, and is, in this case, possibly only a stage in the complete silicification of the rock.

The oolitic iron-ores of Jurassic age in Lorraine, as described by Bleicher (14), have characters which may probably be taken as typical of many other occurrences. As regards their origin the author expresses no opinion, but contents himself with quoting Braconnier's supposition that the material (now hæmatite) was deposited as chalybite brought in solution by water containing excess of carbonic acid. Each grain seems to consist of concentric coats of ferric oxide, more or less argillaceous, investing a central grain. The latter is often organic, consisting of a fragment of a bryozoan, foraminifer, or shell, more or less mineralised and impregnated with iron. The grains, when treated with acid to remove the hæmatite, and heated, became white or grey and transparent. They then consisted merely of a nucleus of siliceous material surrounded by numerous delicate concentric shells, some thin and clear, others cloudy

and enclosing minute sand-grains. These shells were found to be of silica, soluble in caustic soda. With a high magnification they showed very minute rods, which, however, might probably be foreign bodies introduced during the preparation of the specimens.

It is evident that since bedded iron-ore deposits may admittedly be formed either directly as such or indirectly by the transformation of limestones, the origin of any particular deposit must be a question to be decided by such evidence as is available for the case in question. Some of the criteria which may be applied in discussing the question are well brought out in the papers of Kimball and Smyth on the Clinton ores. For instance, Smyth, in maintaining the direct formation of these rocks, points to the rarity or absence in the wide area discussed by him of chalybite, which, on the metasomatic theory, must be formed as a stage in the transformation of limestone to hæmatite. He considers that, if this latter theory were the true one, there should be, at least in some places, ferrous carbonate remaining to mark the intermediate stage of the transition. The normal ore is a red hæmatite, usually of earthy aspect, but sometimes quite compact and with an almost metallic lustre. One type consists largely of fragments of bryozoa, corals, crinoids, brachiopods, etc., coated by, and more or less converted to, hæmatite. The oolitic type is less abundant.

It will be observed that Smyth himself admits some extent of metasomatic change, at least as regards the organic fragments in the Clinton ores. The replacement especially of bryozoan and coral remains is a common feature in many limestones, *e.g.*, in the Carboniferous of the Bristol district. Again, it is not uncommon to find oolitic grains more readily affected than their matrix, and in many of the English oolitic iron-ores we may observe that the grains have been converted into hæmatite or limonite, while the matrix is only in the condition of chalybite.

The metasomatic processes in calcareous rocks which result in what may be termed silicification open out a number of interesting questions which cannot conveniently be treated in the present contribution, and the apparent re-

placement of limestones in some areas by ores of manganese, zinc, etc., must also be left to a future occasion.

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THE DECOMPOSITION PRODUCTS OF PROTEIDS.

AN intimate knowledge of the constitution of Proteids would lead to such important and far-reaching physiological results, that many observers have been induced to undertake lengthy investigations with a view, if possible, to solve or to throw some light upon this difficult problem.

A question such as this, purely chemical as it is in its bearings, must be attacked from a chemical standpoint by a study of the cleavage products of proteids, and just as our knowledge of simpler organic bodies is only placed upon a sure basis by a successful synthesis, so, too, the constitution of the proteid molecule can only be finally and satisfactorily determined by the synthesis of albuminous substances. Before this can be effected it is, however, necessary to ascertain what substances are formed, and the quantities in which they are produced by the disintegration of various proteids.

It is chiefly to a study of this nature that experimenters have as yet applied themselves. At the outset we may classify the experiments as falling into two main groups, the first designed with a view to determine the series of changes a proteid undergoes during its transit through the body, the second with the object of investigating the chemical substances produced as cleavage products by artificial means *in vitro*. In the first category the progress made has been but slight, because of the great and obvious difficulties encountered at every step. We know the end products of the full series of changes, *viz.*, carbonic anhydride and water from the carbon and hydrogen respectively, and from the nitrogen urea, uric acid, ammonia, and other more complex nitrogenous products, occurring either in small quantities or only under special conditions. These result from changes within the tissues and cells of the body, but with regard to the intermediate substances occurring between these at the one extreme, and the com-

plex proteids of the blood and tissues at the other, we are still in the region of conjecture.

There is, however, another series occurring in the alimentary tract which has yielded more plentiful results. As early products we have been taught to distinguish proteids, albuminates and peptones, and many simpler nitrogenous bodies like leucine and tyrosine.

It is mainly, however, from changes brought about by the treatment of proteids with varying reagents that we have gained our greatest knowledge on this subject. In this respect the aim must be to isolate all the products produced during the various decompositions, and so obtain a complete series which will show us, step by step, the succession of changes the proteid has undergone. It is my intention to indicate in this paper how some of these steps have been attained, and to point out the arguments which may be based upon the knowledge thus gained.

Most of the researches as yet carried out have investigated the early or the final products. The changes produced by the first action of the different reagents have resulted in the formation of albuminates, proteoses, and peptones. Into these changes it is not necessary to enter in detail. In considering the final products formed, it will be convenient to arrange the subject under the headings of the different methods employed.

1. *Alkalies.* Mulder[1] treated albumin with caustic potash, and obtained a body to which he gave the name "protein," and which he considered as the base of all albuminous material. He also described various compounds of this substance such as sulpho-proteic acid, protein, etc., but most of his assertions have been disproved by Liebig,[2] who showed that "protein" is a body which we now know as alkali-albuminate. Pavy states that by the action of caustic potash upon albumin he has been able to split off a body of an amylose nature, which on further treatment yields a reducing sugar.

[1] *Journ. f. praktische Chemie*, vol. 16, p. 129, and vol. 17, p. 312; also Liebig's *Annalen der Chemie*, vol. 31, p. 129.

[2] Liebig's *Annalen der Chemie*, vol. 57, p. 132.

O. Nasse introduced the method of boiling proteids with a strong solution of barium hydrate, a method which, elaborated by Schützenberger, has yielded many valuable results. Nasse[1] discovered that under these circumstances some of the nitrogen was disengaged as ammonia, but that this accounted (in all cases with the exception of gluten) for but a small percentage of the total nitrogen. As a result of his experiments he came to the conclusion that the nitrogen of a proteid is held under different conditions; some which is readily eliminated by the alkaline earth is in the form of an amide, some is similar to the nitrogen of a creatine group, but the major part, that which is unaffected by this treatment, is in the form of an amido-acid.

Schützenberger[2] in a series of researches extending over many years has considerably amplified this method. By varying the conditions of temperature and pressure, of amount of barium hydrate employed, and the length of the time of treatment, he has obtained differing results. In his earlier researches he employed coagulated albumin from egg-white which was thoroughly washed with water, alcohol and ether. Weighed amounts of this were treated with quantities of crystalline barium hydrate varying from two to six times its weight, and with water in a closed iron vessel, the whole being heated to temperatures ranging from 100° C. to 250° C., and during times which varied in different experiments from eight to 120 hours. A very large number of such experiments were carried out, and he was able to confirm Nasse's results with regard to the quantity of ammonia given off. Extending his observations he found (i.) that nitrogen to the extent of about 1 per cent. of the total weight of albumin was given off as ammonia by boiling for about half an hour at ordinary pressures; (ii.) that another 1 per cent. is slowly given off by continued boiling for 120 hours, and that this second result can be

[1] *Chem. Centralblatt*, p. 137, 1873. And Pflüger's *Archives*, vol. vi., p. 589; vol. vii., p. 139; and vol. viii., p. 381, 1872.

[2] *Ann. de Chim. et de Physique*, 5th series, vol. 16, p. 289 (1879). *Comptes Rendus*, vol. 101, p. 1267; vol. 102, p. 289; vol. 106, p. 1407; and vol. 112, p. 189. *Bulletin de la Soc. Chim.* (1875), and vol. 24.

more easily obtained by treating with three parts of barium hydrate at 120° C. for six to eight hours ; (iii.) a third 1 per cent. is given off by treating with two parts barium hydrate for twelve hours at 150°, and (iv.) a fourth 1 per cent. is obtained by heating with excess of barium hydrate for several hours at 260°.

He next found that accompanying these four stages of the decomposition there were different cleavage products obtained. First, there were some insoluble barium salts, *viz.*, carbonate, oxalate, phosphate and sulphate. On calculating the quantities of the carbonate and oxalate formed, he arrived at the interesting result, that they were present in just those proportions to confirm the hypothesis, that, with the ammonia set free, they had existed in the proteid molecule as a urea, and oxamide radicle. The barium carbonate and oxalate, moreover, were formed at different stages of the elimination of ammonia, and by combining the results obtained, he was led to the view, that the first amount of ammonia came from one of the amide radicles of the oxamide, the second corresponded especially to the urea, and the third to the remaining nitrogen of the oxamide (present then as oxamic acid).

After precipitating the barium with carbonic anhydride and sulphuric acid, he obtained, by distillation in a partial vacuum, a small quantity of acetic acid mixed with traces of formic acid, and an essential volatile oil which he identified as pyrrol contaminated with smaller quantities of methyl- and ethyl-pyrrol.

The remaining bodies did not volatilise nor sublime at low temperatures, and these he termed the "résidu fixe". By contrasting the elementary analysis of this with that of the original albumin, and taking into account the quantities of the above-mentioned substances, he was enabled to prove that the essential action of the barium hydrate was one of hydrolysis.

For the qualitative analysis of the "résidu fixe" he relied entirely upon processes of isolation by repeated crystallisations from water, alcohol, or ether, after he had spent much time in endeavours to effect the same object by precipita-

tion with basic and neutral lead acetate, or mercuric nitrate, but without success.

Employing this method he showed that the whole of the "résidu fixe" consisted of amido-acids. These are of two classes :—

(A) In the first, which comprises over 80 per cent. of the total weight, the ratio of N : O is equal to 1 : 2, and consists of bodies of the following classes :—

(i.) Amido-acids of the series $C_nH_{2n+1}NO_2$. These he termed leucines. Of these were found Alanine (C = 3) in small quantities only, Propalanine or amido-butyric acid (C = 4), Butalanine or amido-valeric acid (C = 5), both in considerable quantities, and Leucine or amido-caproic acid (C = 6) in very large quantities. It is to be noted that glycocine (amido-acetic acid) was not found.

(ii.) Amido-acids of the series $C_nH_{2n-1}NO_2$. These belong to the acrylic acid series, and Schützenberger called them Leucëines. Here, too, the term occurring most abundantly is (C = 6) "caproic Leucëine," but bodies corresponding to C = 5 or 4 were also found.

(iii.) Amido-acids of the form $C_nH_{2n}N_2O_4$ or some multiple of this. To these bodies he gave the name of glucoproteins. The most abundant of these are those in which C = 9 and C = 7 or some multiple, but others where C = 11, 10, 8 have also been isolated.

(B) In the second the ratio of N : O is 1 : 3 or 1 : 4 or 2 : 5. It only contains about 16 per cent. of the total weight. In this class are found :—

(i.) Tyrosine $C_9H_{11}NO_3$ or oxyphenyl-amidopropionic acid. The amount of this is about 3·5 per cent.

(ii.) Nearly the same quantity of a body Tyroleucine $C_7H_{11}NO_2$.

(iii.) Very small quantities of glutaminic acid $C_5H_9NO_4$. This is an amido-acid (optically inactive) of one of the pyrotartaric acids (glutaric).

Of these substances Schützenberger found varying

quantities, according to the degree to which the hydration had been carried out. If this had been thorough the final bodies, *viz.*, the leucines and leucëines, were found, but if it had only been partial large quantities of glucoproteins were obtained. He also found evidence of combinations of these with leucines or leucëines to form still more complex bodies. He therefore concludes that the albumin molecule under the action of barium hydrate loses ammonia, carbonic anhydride, acetic and oxalic acid, and becoming hydrated forms first glucoproteins, mainly $C_9H_{18}N_2O_4$ or more probably a multiple of this, and on further action these become converted into bodies of the leucine and leucëine type.

He has further extended his observations to other proteids and shown that they give almost identical results.

Under the same treatment gelatine, icthyocoll and ossein were found to yield the same bodies, with the addition of amido-acetic acid (glycocine) in very large quantities (20-25 per cent.).

2. *Acids.* Prolonged heating of proteids with dilute acids was found by Neumeister[1] to result in hydration and the production of proteoses and peptones. I have observed the production of the same bodies by the action of strong acids acting for several days at the temperature of the room. The prolonged boiling with a strong solution of sulphuric or hydrochloric acids yielded ammonium salts, leucine, tyrosine, aspartic and glutaminic acid. This method had the great disadvantage that many strongly coloured bodies appeared, and to avoid this Hlasiwetz and Habermann[2] introduced the important modification of heating with strong hydrochloric acid and stannous chloride, by which pale yellow solutions were obtained which showed no trace of charring. At the end of the process most of the stannous chloride was found converted into stannic chloride, showing that a certain amount of reduction had occurred. Casein was the proteid at first employed by them in their researches, and from the liquid so obtained they were able to isolate :—

[1] *Zeitschr. für Biologie*, vol. 23, p. 381.

[2] Liebig's *Annalen der Chemie*, vol. 169, p. 150, 1873.

(i.) Ammonia.
(ii.) An amido-acid of the acetic series, *viz.*, leucine.
(iii.) Two amido-acids of the acrylic series, *viz.*, asparaginic acid $C_4H_7NO_4$ (amido-succinic acid), and glutaminic acid $C_5H_9NO_4$ (amido-pyrotartaric acid), both of which were found in considerable quantities. On one occasion the glutaminic acid amounted to as much as 29 per cent. of the weight of dry casein used.
(iv.) Tyrosine or oxyphenyl-alanine.

It is thus seen that, as with the action of barium hydrate, the result of this decomposition is to lead mainly to the formation of amido-acids.

Ritthausen[1] was the first to separate glutaminic and asparaginic acids from proteids. He obtained glutaminic acid from legumin and conglutin, and Kreusler examined animal proteids treated in a similar manner, but failed to isolate it. He consequently suggested the view that it was a typical product of the decomposition of vegetable-, but absent from animal-proteids. Hlasiwetz and Habermann,[2] however, obtained it in considerable quantities from casein, albumin, and several other proteids. It is interesting to note that the glutaminic acid here isolated is lævorotatory, whilst that obtained by Schützenberger was only found in small quantities and inactive. By boiling the active form with barium hydrate, however, it becomes converted into the inactive form, and further, this inactive form, or that obtained by Schützenberger's process, if allowed to ferment under the action of *Penicillium glaucum*, becomes transformed into the optically active modification.[3]

Employing the same method, Horbaczewski[4] obtained from gelatin, leucine, glutaminic acid and glycocine, but

[1] *Journ. f. praktische Chemie*, 107, p. 218, and 99, p. 454.
[2] *Anz. der K. Akademic zu Wien*, ix., p. 114, 1872.
[3] *Berichte der deutschen Chemischen Gesellschaft*, 17, 388. Changes in the optical varieties of leucine have been produced by exactly similar methods.
[4] *Sitz. der K. Akad. zu Wien*, bd. 80, Abthlg. 2, s. 101, 1880.

obtained no asparaginic acid nor tyrosine. Hofmeister[1] has, however, obtained small quantities of asparaginic acid. From elastin, Horbaczewski[2] has obtained by the same method ammonia, leucine, tyrosine, glycocine and amido-valerianic acid $C_5H_{11}NO_2$ (butalanine), but no glutaminic nor asparaginic acids. Siegfried, working on a substance he obtains from reticular tissue, and which he calls "reticulin," obtained ammonia, sulphuretted hydrogen and amido-valerianic acid, but no tyrosine nor glutaminic acid.

By the employment of this same method, Drechsel has discovered two new bodies, lysine and lysatinine, which are of especial interest. He was induced to take up this question by a consideration of the quantities of the different nitrogenous bodies which had been obtained. Assuming that only one half of these bodies had been isolated by the methods hitherto employed, there still remained about 30 per cent. of the original nitrogen to be accounted for. Now contrasting Hlasiwetz and Habermann's results with those obtained by Schützenberger, Drechsel[3] points out that carbonic anhydride was produced by the latter method but never by the former, and he therefore argues that there must be some other body which should be capable of isolation, and which on treatment with barium hydrate will yield carbonic anhydride.

We have already seen that Schützenberger[4] considers this would be of the nature of a ureïde. Considering it probable, therefore, that this substance might be of a basic nature, he determined to first ascertain whether he could isolate it as a precipitate by phosphotungstic acid, being led to employ this reagent on account of its known value as a precipitant of bodies of an alkaloidal or basic nature. In this expectation he was completely successful and obtained a very bulky precipitate from which he was able to isolate two bases, which he named lysine and lysatinine.

Lysine has a formula $C_6H_{14}N_2O_2$ which is homologous

1 *Zeitschr. f. physiol. Chemie von Hoppe-Seyler*, vol. ii., p. 299.
2 *Sitz. der K. Akad. zu Wien*, bd. 92, Abthlg. 2, s. 657, 1886.
3 *Der Abban der Eiweiss-stoffe du Bois' Archiv*, p. 248, 1891.
4 *Loc. cit.*

with Jaffé's[1] ornithin $C_5H_{12}N_2O_2$, which is probably diamido-valerianic acid. Drechsel considers therefore that probably lysine is a body of this class (diamido-caproic acid, for instance).

If this base be heated with strong hydrochloric acid up to a temperature of 150° C., no change occurs; but on heating it with barium hydrate to 120° or 130° it is decomposed with the production of barium carbonate. It can, therefore, account for some of the carbonic anhydride observed in Schützenberger's experiments.

The chief interest centres, however, round the second base, lysatine or lysatinine, which has a formula either $C_6H_{13}N_3O_2$ or $C_6H_{11}N_3O$, and is a homologue of either creatine or creatinine $C_4H_9N_3O_2$ or $C_4H_7N_3O$, according as the first or second formula is taken. It is obtained pure as a double salt of its nitrate in combination with silver nitrate, and this double salt when boiled with barium hydrate for twenty-five minutes yields urea. This is a decomposition exactly analogous to that of creatinine under the same circumstances. From ten grammes of the double salt he obtained about one gramme of urea, a considerable quantity when one considers that urea itself under these circumstances is quickly broken up into ammonia and carbonic anhydride. The great importance of these facts rests in that this is the first time urea has been obtained as a direct decomposition product of a proteid.

Béchamp,[2] viewing metabolism as essentially a process of gradual oxidation, considered he ought to get urea by acting upon proteids in such a manner. He therefore treated dried egg-white with potassium permanganate and stated that he had been successful in obtaining small quantities of urea. This result was contradicted by Stædeler,[3] Lœw,[4] and Tappeiner,[5] though confirmed by Ritter.[6] The matter was, however, finally settled by the experiments of

[1] *Ber. der deut. chem. Gesellschaft*, vol. x., p. 1925.
[2] Liebig's *Ann. Chem.*, vol. 100, p. 247, 1856.
[3] *Journ. f. Prak. Chemie*, vol. 72, p. 251, 1857.
[4] *Ibid.* (2), vol. iii., p. 289, 1871.
[5] *Ibid.* (2), vol. iv., p. 408, 1871.
[6] *Comptes Rendus*, 73, 1219.

Lossen,[1] who found that the substance Béchamp and others had obtained was not urea, but guanidine, which is readily mistaken for it. This guanidine might have originated from small quantities of xanthine present as an impurity in the egg-white employed, for xanthine on oxidation with hydrochloric acid and potassium chlorate produces guanidine. On the other hand Drechsel[2] points out that creatine is a methyl-guanidine acetic acid and that when treated with mercuric oxide it yields methyl guanidine; and assuming lysatinine to be a homologue of creatinine, the guanidine obtained by Lossen may have come from that, and Drechsel hopes to be able, by treatment with mercuric oxide, to obtain it from lysatinine. Drechsel is careful to point out that the urea obtained by his method is not a result of oxidation, but of hydration processes. The stannous chloride effectively prevents oxidation, and, moreover, the action of the barium hydrate on lysatinine is again a hydration.

The remaining products obtained from the lysatinine have not yet been isolated.

Drechsel, therefore, considers that one possible source of urea as a result of proteid metabolism may be lysatinine. But supposing that all the proteid assimilated yielded the proportion of lysatinine, and then of urea, that he found in his experiment *in vitro*, even then only about one ninth of the total urea excreted will be accounted for.

There is, however, another point which has not been noticed, but which I think gives us a reason why we should not expect to find urea among the decomposition products of proteids. The whole of the knowledge we at present possess tends to show that urea is not formed in those positions in the body where proteid disintegration is occurring, *viz.*, the tissues, but that it is formed in one position only, the liver, and there too mainly, if not exclusively, from the very simplest nitrogenous bodies, ammonium salts. Looked at from this point of view the series of

[1] *Ann. Chem. Pharm.*, vol. 201, p. 369, 1880.
[2] *Loc. cit.*

changes a proteid seems to undergo within the tissue cells result in the formation of ammonia, not of urea. This ammonia passing into the lymph and blood combines with carbonic acid to form either carbamic acid or carbonate of ammonium. The production of carbamic acid especially occurs if the fluid is alkaline. Drechsel [1] has found it in the blood-serum, and Drechsel and Abel [2] have found it in horses' urine.

Schröder's experiments have shown that ammonium carbonate added to blood circulating artificially through an isolated liver, is directly converted into urea. This is confirmed, and the view that this is the normal method within the body is upheld by the experiments in which the urea and ammonium salts have been quantitatively determined in the urine. When a strong mineral acid is administered so as to render the blood plasma less alkaline, more and more of the eliminated nitrogen appears as the ammonium salt of a mineral acid (sulphate, chloride, etc.), and less as urea. And we know, further, that ammonia presented to the liver cells, in combination with strong mineral acids, is not altered by them.

The changes occurring, then, to proteids within the body are first hydrolisation, and then the carbon is split off, atom by atom, and burnt to form carbonic anhydride. That this gradual disintegration is possible is shown by a previous research of Drechsel's [3] relating to the action of strong alternating currents upon caproic acid. The alternating current acts upon the solution so as to produce rapidly changing series of oxidations and reductions. By an examination of the results Drechsel found that from the caproic $CH_3.(CH_2)_4CO.OH$, oxy-caproic $CH_2.OH.(CH_2)_4CO.OH$ acid arose by oxidation, from this adipic acid $CO.OH(CH_2)_4CO.OH$ arises by oxidation, and on oxidation of this oxy-valerianic acid $CH_2OH(CH_2)_3CO.OH$ and carbonic anhydride are produced, and so on till all the carbon atoms are burnt off as carbonic anhydride.

Again, from oxy-valerianic acid $CH_2OH.(CH_2)_3CO.OH$

[1] *Sitz. der Königl. Sächs Gesell.* Sitzung vom., 21 April, 1875.

[2] *Du Bois' Archiv*, p. 236, 1891.

[3] *Journ. Prak. Chemie* (2), vol. 34, p. 135.

by reduction valerianic acid $CH_3(CH_2)_3COOH$ is produced. All these bodies have been identified by Drechsel, and he moreover shows that the process can be carried out to the end, when all the carbon appears as carbonic anhydride, the hydrogen as water.

On similar treatment of an amido-acid a similar series is observed, the nitrogen being ultimately found as ammonium carbonate.

Further the same process will convert ammonium carbonate into urea.[1]

This series of changes will thus help us to understand the formation of several terms in the homologous series isolated by Schützenberger, and the contrast of but few of the terms of the series isolated after treating proteids with strong acids.

Drechsel obtained lysine and lysatinine from casein, but working under his direction several of his pupils have discovered them among the cleavage products of other proteids. Fischer[2] obtained them from gelatin. Siegfried obtained them from conglutin[3] and from reticulin.[4] Siegfried working with conglutin also obtained a body of formula $(C_8H_{16}N_2O_4)_n$ of sweet taste, and answering to one of the bodies described by Schützenberger as a glucoprotein.

3. *The action of oxidising agents.* Treatment with nitric acid has yielded oxalic, fumaric, and saccharic acids. By its early action a yellow insoluble substance, xanthoproteic acid, is first produced, but this gradually dissolves, and finally there is produced para-oxybenzoic and oxybenzoic acids. These latter are probably due to the oxidation of tyrosine.[5]

Oxidation with manganese dioxide and sulphuric acid, or with potassium bichromate and sulphuric acid, has yielded—

[1] Drechsel, *Journ. f. Prak. Chemie* (2), vol. 22, p. 476.

[2] *Inaugural Dissert.*, Leipzig, 1890; and *Du Bois' Archiv*, p. 265, 1891.

[3] *Ber. d. deut. chem. Gesell.*, vol. xxiv., p. 418.

[4] *Ueber die chemischen Eigenschaften des reticulirten Gewebes* (*Habilitationschrift*), Leipzig, Dec., 1892.

[5] See Baumann, *Ber. d. deutsch. Chem. Gesellschaft*, 12, 1453.

(i.) Formic acid, acetic acid and the homologues of these fatty acids up to caprylic acid $C_8H_{16}O_2$.
(ii.) The aldehydes of these acids.
(iii.) The nitriles of acetic, proprionic, valeric, and butyric acids and hydrocyanic acid.
(iv.) Benzoic acid and benzoic aldehyde.

When oxidised with chlorine water they yield among other bodies, fumaric and oxalic acids, and chlorazol.

Oxidised by bromine water at high temperatures in a sealed tube, Hlasiwetz and Habermann[1] isolated from among the products formed carbonic anhydride, oxalic acid, ammonia, bromanil ($C_6Br_4O_2$), bromoform ($CHBr_3$), monobrombenzoic acid, mono- and di-bromacetic acids, tribromamidobenzoic acid ($C_6HBr_3(NH)COOH$), asparaginic and malaminic acids, leucine, and leucimide ($C_6H_{11}NO$). No tyrosine was obtained.

Bleunard,[2] working with the results of the action of bromine, has been able to confirm the production of Schützenberger's glucoproteins.

4. *By dry distillation* of proteids a complex oil, "Dippel's oil," is produced which contains among other substances :—

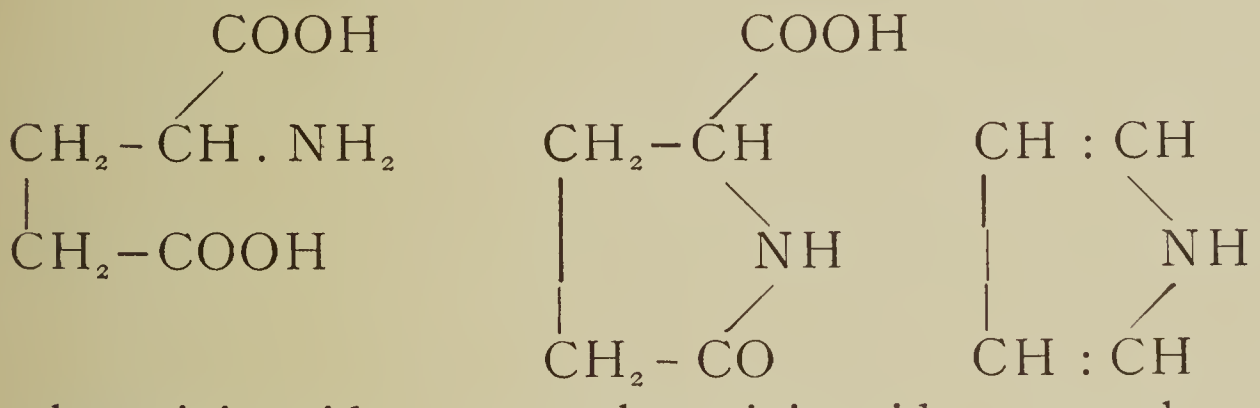
(i.) Hydrocarbons of the fatty series.
(ii.) Ammonium salts of the fatty acids, *e.g.*, butyric, valeric, caproic.
(iii.) Nitriles of this series from propyl nitrile to stearyl nitrile.
(iv.) Ketones of this series.
(v.) Carbonic anhydride.
(vi.) Ammonia and amines of fatty acid radicles, *e.g.*, methylamine, propylamine, butylamine.
(vii.) Hydrocarbons of the benzene series.
(viii.) Amines of this series. Aniline and its homologues.
(ix.) Phenol and its homologues.
(x.) Homologues of the pyridine group of bases:

[1] Liebig's *Ann. d. Chem.*, 159, p. 304, 1871.
[2] *Comptes Rendus*, vol. 90, p. 612, and vol. 92, p. 458.

Pyridine ($C_5H_5N_5$), Picoline (C_6H_7N), Lutidine (C_7H_9N), and Collidine ($C_8H_{11}N$).

(xi.) Pyrrol. Of feebly basic properties.

Perhaps the most interesting of these are the pyridine group and pyrrol; the former because they have been shown to take a part in the molecular formation of many vegetable alkaloids, *e.g.*, piperidine, cinchonine, etc. As an example which will show us the manner in which the bodies described as decomposition products of proteids by acids and alkalies could become converted by distillation into bodies of closed rings, such as the last two mentioned classes, I will take the conversion by heat of glutaminic acid into pyrrol. At 190° dry glutaminic



glutaminic acid. pyroglutaminic acid. pyrrol.

acid is converted into pyroglutaminic acid, and at a higher temperature this in its turn becomes converted into pyrrol.[1]

5. *Decompositions occurring by ferment action.* First among these we have the earlier stages of hydration of the proteid, *viz.*, the proteoses and peptones; but in addition to these it has been shown that the action of trypsin may extend further. The other substances known to be produced may be classified into :—

(i.) Bases. Lysine and lysatinine have been shown by Hedin,[2] working under Drechsel, to occur among these.

Hirschler[3] obtained evidence of the production of ammonia, a result confirmed by Stadelmann[4] under conditions precluding the possibility of putrefaction.

[1] *Ber. der deutsch. chem. Gesell.*, xv., 1222.
[2] *Du Bois' Archiv*, p. 273, 1891.
[3] *Zeitschr. f. physiol. Chem.*, vol. x., p. 302, 1880.
[4] *Zeit. f. Biologie*, vol. xxiv., p. 261, 1888.

(ii.) Amido-acids. Of these leucine, amido-valerianic (butalanine), amido-succinic (aspartic), and amido-pyrotartaric (glutaminic) acids have been isolated.

(iii.) Aromatic bodies, tyrosine or oxyphenyl-alanine. It is thus seen how closely these results correspond with those obtained by artificial decomposition.

Within the intestine many changes occur which have been found due to decompositions under the influence of bacterial action. The same products, as above enumerated, are first formed, but by further and different changes other bodies rapidly arise. Of these we may mention—indol, skatol, skatol-carbonic acid, oxyphenyl propionic acid, oxyphenyl-acetic acid, phenyl propionic and phenyl-acetic acids, para-kresol and phenol, and as simpler bodies carbonic anhydride, water, ammonia, hydrogen, and sulphuretted hydrogen, in addition to amido-acids of the fatty series and the fatty acids themselves.

In this case certainly the most interesting point to note is the production of so many derivatives containing the benzene nucleus. The indol group has never been obtained from a proteid molecule by any other method.

T. Gregor Brodie.

Science Progress.

No. 20. October, 1895. Vol. IV.

RECENT PROGRESS IN THE STUDY OF ALLOYS.

THE systematic knowledge that we possess concerning the chemical relations of the compounds formed by non-metallic elements with each other, and with the metals, renders it surprising that we should know so little about the nature of alloys, that is, of bodies in which only metallic elements occur.

A great deal of attention has been given to this class of substances, and many isolated facts are known concerning the properties of individual alloys, but general laws have yet to be discovered. Much of our knowledge is, in fact, the accumulated tradition of technical workers, and it would not be a far-fetched illustration to compare the present state of the chemistry of alloys to that of chemical science in general before the time of Boyle.

It is easy to point out some of the special difficulties to which this state of affairs is due. When non-metals combine with each other or with metals, the products, as a rule, differ widely in properties from the original substances; hence the separation of the product in a pure state is a comparatively easy task. But this is not the case when two metals combine to form a compound: hitherto no alloy has been discovered which is more volatile than the metals composing it, and, though differences in fusibility exist, they are hardly sufficient to permit of the isolation of pure substances from mixtures.

Moreover, compounds of two metals retain a metallic appearance, and electrolysis, which has thrown so much light on the other parts of chemistry, here fails us completely; for an alloy, whether it be a compound or a mixture, appears to conduct without separation into its component metals.[1] The fact that all metals and their alloys are opaque, and that on account of the high temperature at which they melt vessels of clay or other opaque substance must in general be used to contain them, enormously increases the difficulty of experiment. We cannot note by eye the stage at which a liquid ceases to be homogeneous, or study the appearance of the bodies which crystallise out one after another from solution.

Hence the problem of the nature of alloys, although it is in part a chemical one, has hitherto been most successfully attacked by physical methods. We shall therefore begin by briefly reminding our readers of Matthiessen's work, and then go on to the results arrived at by the late Professor Alder Wright, Professor Roberts Austen, and by Heycock and Neville. The main outcome of this work can be summed up in the statement—that mixtures of liquid metals obey those general laws of solution which have been so brilliantly developed in the last ten years.

We shall then consider the more purely chemical attempts that have been made to isolate alloys of definite formula, work principally due to Deville and Debray, Schützenberger, Osmond and Werth, Guillemain, Levy, Joanniss, Charpy, and Le Chatelier.

Matthiessen's remarkable experiments on the electrical conductivity of solid alloys drawn into wire[2] must ultimately find a simple explanation; but partly, perhaps, because solid alloys are more complex than liquid ones, no satisfactory explanation has yet been given, and therefore these experiments will not be described in the present paper. The same must for the present be said of Weber's work on the changes in the electrical resistance of alloys during

[1] Obach, *Pogg. Annalen. Ergänzungs*, bd. vii., p. 280, 1876; and Elsässer, *ibid.*, N.F., bd. viii., p. 455, 1879.

[2] *B.A. Report* and *Phil. Trans.*

melting,[1] and of Le Chatelier's interesting experiments on the connection between the temperature and the resistance of certain alloys.[2]

Matthiessen's theory that certain metals when added, even in very small quantity, to another metal bring the latter into an allotropic state, has been often quoted, but cannot be regarded as having borne fruit up to the present time. On the other hand, his distinction between alloys—which are: (1) a homogeneous solution of one metal in another; (2) a homogeneous solution of a definite chemical compound of the two metals in the one which is present in excess; (3) a mechanical mixture of two different liquids, each containing both metals, but in different proportions —has been a most useful one.

As an example of class (3) he gives the case of a mixture of equal parts of water and ether. This, if well shaken, would for a short time appear fairly homogeneous, although it really consisted of drops of water containing some ether mingled, but not mixing, with drops of ether containing some water; such a mixture would on standing separate into two layers.

It is probable that many alloys are of this class, although the separation into two layers is a slow one which is very far from being complete when the alloy is solidified by rapid cooling. It seems improbable that any alloy of this class can be thoroughly satisfactory for mechanical purposes, or can be sufficiently homogeneous to make its properties worth studying. For such an alloy when solid would be a sort of conglomerate or concrete, and although such a texture might not be inconsistent with mechanical strength if a sufficient uniformity of grain could be ensured, yet it would be difficult to prevent parts of the mass differing in character. Moreover, differences in the coefficients of expansion of the two materials would be a cause of weakness. Professor Roberts Austen says that alloys of iron and aluminium containing from 40 to 60 per cent. of aluminium disintegrate spontaneously into clean metallic grains when

[1] *Annalen*, N.F., bd. 34, p. 576, 1888.

[2] *Zeitschrift für Physikalische Chemie*, bd. viii., p. 183, 1891.

kept for some months. This may be an extreme case of the weakness inherent in a conglomerate alloy.

A study of the proportions and temperatures at which the metals when melted in pairs form homogeneous liquids, ought therefore to be the first step in a scientific study of alloys. Matthiessen was aware of the importance of this subject, and made experiments on the miscibility of pairs of metals; but the later experiments of Alder Wright are more accurate and are the only ones we shall quote.

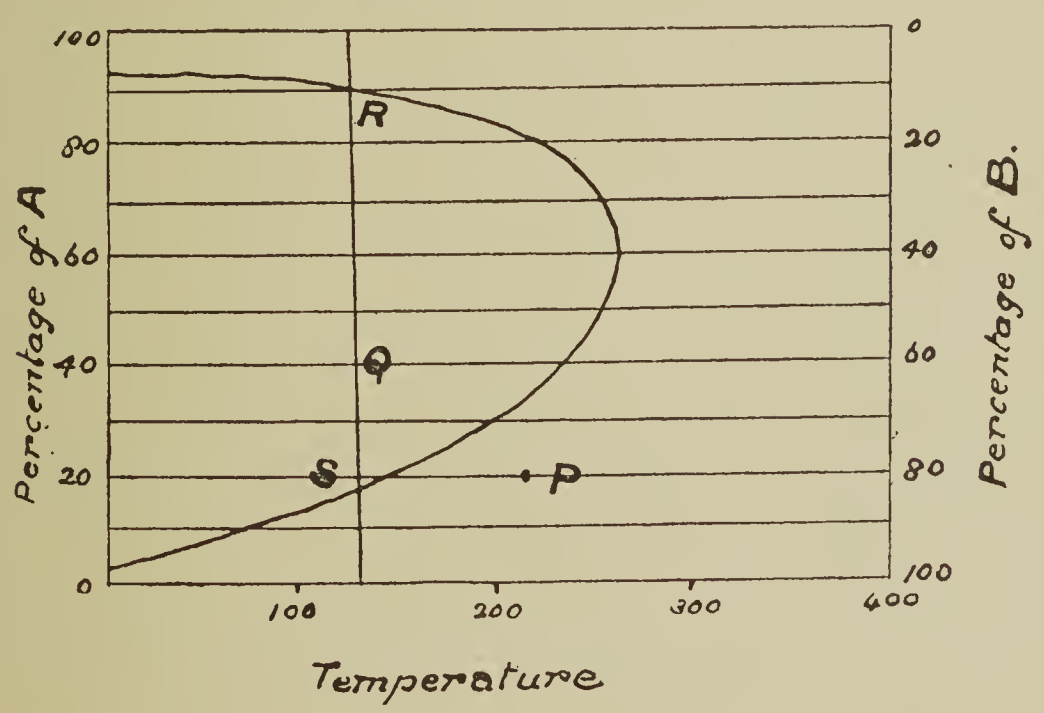
The behaviour of partially miscible liquids to one another has, however, been much more perfectly studied by Alexejew[1] and Konovalow.[2] These experimenters did not examine metallic solutions, but it is almost certain that the general conclusions they arrived at are applicable to alloys. The facts, so far as they affect our subject, can be very briefly stated. If a liquid B is gradually added to another liquid A and the mixture is sufficiently stirred, we shall at first get a homogeneous liquid, which may for convenience be called a solution of B in A. Continued addition of B will make this solution stronger, but it will remain homogeneous until A is saturated with B. A further addition of B, or the removal of some A by evaporation or freezing, will now cause the appearance of a second layer of liquid not miscible with the first. This new liquid may appropriately be called a saturated solution of A in B. A further addition of B will not alter the composition of these two layers, but will cause the second layer to increase at the expense of the first, and if we add enough of B the first layer will disappear and we shall have only the saturated solution of A in B, while with a still further addition of B we shall obtain an unsaturated solution of A in B. In other words, there is an infinite number of different proportions in which A and B may be shaken up together so as to give two layers of liquid, and in all cases the composition of these two layers will be independent of the proportion of A and B employed. These two layers of

[1] *Wied. Annalen*, 28, 305, 1886.
[2] *Ibid.*, 14, 219, 1881.

liquid, which can exist in presence of each other without change, may conveniently be termed *conjugate*.

The above applies to experiments conducted at a constant temperature. If we work at a higher temperature the compositions of the two conjugates will be altered. In general, raising the temperature will cause B and A to become more soluble in each other; so that the two conjugates approach each other in composition as the temperature rises, until finally a critical temperature is reached at which the two conjugates have the same composition, and, therefore, are indistinguishable from one another. At this and all higher temperatures the system will consist of one homogeneous liquid, that is to say, A and B will be miscible with each other in all proportions. Conversely, if A and B are mixed at a temperature above the critical, and the resulting homogeneous liquid be gradually cooled, a temperature will be reached at which the liquid will separate into conjugate layers. If temperature be plotted horizontally and the percentage composition of the mixture vertically, the composition of every possible pair of conjugates will be given by points on a curve, as in figure 1; and

FIG 1.



the two points lying on the same vertical will correspond to a pair of conjugates. Every point in the area of the figure corresponds to a definite state of percentage composition and temperature, but the points, such as P, R, or S, which

are on or outside the curve, are the only ones which correspond to a homogeneous solution. A point such as Q, which is inside the curve, corresponds to an imaginary solution, for such a state would break up into the two conjugates R and S which lie on the same vertical as Q. For example, a mixture of copper and tin, containing a large percentage of both metals, is probably a real solution so long as the temperature is sufficiently high ; but, long before solidification commences, it probably begins to separate into two conjugate alloys. This process may, through the action of gravity, result in any degree of heterogeneity, even to the formation of two layers of liquid if the cooling is slow enough.

These facts have been very little studied in metallurgy, except unsystematically by the technical worker, who has often found his bronze liquating or, apparently, separating into its constituent metals as it cools.

Alder Wright[1] has shown that at temperatures in the neighbourhood of 750° C. a mixture of lead and zinc separates into conjugate alloys containing respectively 1·3 per cent. of zinc in the heavier, and 1·57 per cent. of lead in the lighter alloy. He has shown that at 650° C. the zinc-bismuth conjugates contain respectively 14·28 per cent. of zinc, and 2·32 per cent. of bismuth, the increase in solubilities being just noticeable at 800° C. In the case of lead and aluminium at 800° C., the lead layer contained ·07 per cent. of aluminium, and the aluminium layer contained 1·91 per cent. of lead.

No attempt has been made to plot the curve of conjugates, similar to figure 1, for a pair of metals, and so to find the critical temperature of complete miscibility. The research would be a laborious one, but it might be carried out by using Alder Wright's methods, supplemented by a thermo-couple or a platinum resistance thermometer to measure the temperatures.

A research of more immediate importance for the art of metallurgy would be the determination of the composition

[1] *Proceedings R.S.*, 45, 48, 49, 50, 52, 55.

of the conjugate alloys of pairs of metals at the temperatures at which solidification commences; in other words, of the solubilities of the two metals in each other at the commencement of solidification. The writer is disposed to think that these solubilities at the freezing point, the eutectic[1] alloys in fact, give the limiting proportions in which the metals can be usefully alloyed for metallurgical purposes. A few of these solubilities have been published by Heycock and Neville, but as yet only one conjugate of each pair has been studied, and the metal pairs most useful in metallurgy, such as copper-tin, copper-zinc, and copper-aluminium, have not been discussed. A careful study of the cooling curves obtained by Roberts Austen would perhaps give us these solubilities.

Conjugate solutions, such as those above described, have been found by Konovalow[2] to possess a remarkable property. It is this, that if either or both of the components be volatile, then the vapour given off by each of the two conjugates is identical in pressure and in composition, though the two conjugate liquids may be very different from each other. Consequently the two conjugates would begin to boil at the same temperature. This property is not at present of importance in the case of conjugate alloys on account of the low vapour pressure of the metals at the temperatures we can command; but, as Ostwald has pointed out, a corresponding property as to the temperature at which a pair of conjugates would refuse to dissolve either of the metals in the solid form may be important. It seems probable from theoretical considerations, that if the pair of conjugate alloys be formed at so low a temperature, that one of them is saturated with the solid metal A; then the other conjugate is also saturated with A at the same temperature. If heat is now subtracted from either conjugate the first result will be the same, namely, the precipitation of solid A. Continued abstraction of heat will, however, affect the two alloys differently—that richest in A will be

[1] That mixture of two substances which has a lower melting point than mixtures of the same substances in other proportions is said to be *eutectic*.

[2] *Loc. cit.*

gradually converted at a constant temperature into its conjugate, with precipitation of A, while the other conjugate, if heat be taken from it, will slowly cool with precipitation of A.

ON TERNARY ALLOYS.

Practical metallurgists have long ago recognised that the addition of a third metal to an alloy materially alters its properties, and the number of ternary alloys, or mixtures of three metals, that have been devised for special uses is very large, many different alloys being used for the same purpose by different manufacturers. A good deal of light has been thrown on this complicated subject by the researches of Alder Wright.[1] Developing facts previously studied by Duclaux[2] in the case of mixtures of water, ether, and alcohol, he has shown that if we take two partially miscible liquids, such as lead and zinc, in such proportions that the mixture separates into two conjugate alloys, then by the addition of a third metal, such as tin, which is freely soluble in both the others, we can increase the solubility of the lead and zinc in each other. The two conjugates can thus be made to approach each other in composition, and a sufficient amount of tin will make them identical, so that for this and all higher proportions of tin, the three metals will form a homogeneous mixture. The third metal thus acts somewhat like a rise in temperature in Alexejew's experiments (fig. 1).

It is not improbable that the beneficial effect of the third metal in many alloys in use, is due to its thus rendering the mixture of the other two homogeneous. Alder Wright's method of experiment was to melt the three metals together in the desired proportion and pour the mixture into a long narrow crucible of clay, which was maintained for several hours at a constant temperature well above the melting point. The alloy was then rapidly cooled, and the upper and lower parts of the ingot analysed. In those cases where the amount of the third metal was insufficient to

[1] *Loc. cit.*

[2] *Annales de Ch. et Ph.* (5), 7, 264, 1876.

prevent separation, the ingot was seen to consist of two layers having a well-marked dividing surface between them.

His results admit of being very elegantly expressed by a method suggested by Sir George Stokes. Suppose that a mixture be made containing *a* per cent. of A, *b* per cent. of B, and *c* per cent. of C; then, if we draw an equilateral triangle A B C and find the centre of gravity P of masses *a*, *b*, *c*, placed at the corners A B C respectively, the point P will record the composition of the mixture. Conversely, every point in the area of the triangle represents a different mixture. For example, the point P in figure 2 represents a mixture of two parts of A, one of B, and one of C.

Now, Alder Wright's results show that if the percentage

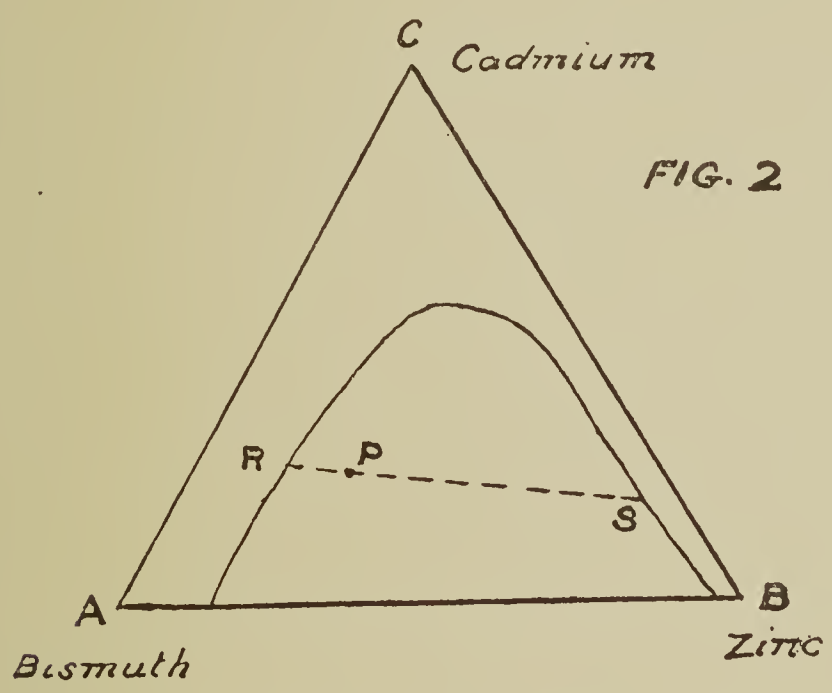


FIG. 2

of the third metal C, which mixes freely with both A and B, and may therefore appropriately be called the solvent, be small enough, that is, if P be near enough to the line A B, and not too near either A or B, then the alloy represented by P separates into two conjugates represented by the points R and S lying on a straight line through P. If a series of mixtures of A, B, and C in different proportions be examined, and the conjugates into which they divide be found by analysis of the upper and lower parts of the ingots, we can plot a series of points, such as R S. The curve passing through all these is called the *critical curve*. Figure 2 gives Alder Wright's critical curve for mixtures of bismuth, zinc, and cadmium at 600° C.

Alder Wright has traced the critical curve for the following cases, the first named metal of each triad being the solvent metal.

Sn, Pb, Zn	
Sn, Bi, Zn	
Sn, Pb, Al	
Cd, Bi, Zn	
Cd, Pb, Zn	
Sb, Pb, Al	
Sb, Bi, Al	
Ag, Bi, Al	
Ag, Bi, Zn	

A critical curve having been determined we can at once predict from it whether a proposed mixture will remain homogeneous or will separate into two layers. If the point corresponding to the mixture be plotted, and lies on or outside the critical curve, the alloy will be homogeneous; if it lies inside, the alloy will split into conjugates. A critical curve must of course be constructed as the result of a series of experiments, carried out at a constant temperature. If the temperature rises, the curve shifts, generally in the sense that the higher the temperature, the smaller is the area inside the curve; that is, the less solvent is needed to make a homogeneous or *real* alloy. The Bi Zn Cd curve given in figure 2 is very sensitive to change of temperature.

If the critical curves of the triads of metals, habitually used together in the arts, were determined at the lowest temperatures compatible with perfect fluidity, we might perhaps find in them a rational basis for predicting the proportions of the metals to be employed in the most useful alloys.

Alloys which were *real*, in the above sense, would probably prove to be mechanically homogeneous when solid; although even in these, a microscopical examination would in most cases show structure.

THE SOLIDIFICATION OF ALLOYS.

In studying the process by which an alloy solidifies observations with the thermometer are of great value. For, so long as the mass of cooling substance remains completely liquid, and abrupt chemical changes do not take place, a thermometer immersed in it will indicate a uniform rate of cooling, but the separation of solid matter will be marked by the evolution of heat, of fusion or solution. If the formation of solid matter is gradual and con-

tinuous, as will be the case when some of the solid is already present, the thermometer will indicate a slower rate of cooling, or, if the system is protected from too rapid loss of heat, the temperature may become constant for a time. But molten metals, like other liquids, show the phenomenon of *surfusion*, that is to say, the liquid, if no solid is present, can be cooled below its proper melting point. It is then in an unstable condition, and sooner or later this state ends by the rapid formation of a considerable amount of solid with the evolution of a corresponding amount of heat. If the system is so arranged that losses of heat by radiation and conduction are slow, the heat evolved at this stage may raise the temperature of the whole mass considerably, the highest temperature attained being at, or very nearly, that at which solid and liquid can exist in permanent equilibrium. This temperature, the freezing point as it is convenient to term it, is thus often indicated by the thermometer with great delicacy.

If we are dealing with a pure metal the freezing point is remarkably constant, the temperature after the period of surfusion is over, remaining absolutely the same until the whole mass is solid. Conversely, if heat be slowly added to a mass of the pure solid metal, the temperature will become constant as soon as fusion commences, the melting and freezing points being absolutely identical.

When the metal is no longer pure, but is alloyed with a small quantity of some other metal soluble in it, the process of solidification obeys the law of Raoult. The metal present in excess, which we may term the solvent, [1] no longer solidifies at its true freezing point; but it does not begin to do so until a lower temperature is reached, the pure solvent separating at continually lower and lower temperatures as the liquid becomes a stronger solution of the dissolved metal.

[1] When two substances are mixed and form a homogeneous liquid, it is difficult to give a rule for deciding which of the two is the solvent. There is a tendency to regard that substance as the solvent which separates out in cooling. But this rule breaks down when we consider the cooling of a saturated solution.

In the case of dilute solutions the depression of the freezing point below that of the pure solvent is found to be directly proportional to the percentage weight of dissolved metal and inversely proportional to its atomic weight. But if we continue to increase the strength of the solution a concentration will be reached at which the solvent is saturated with the dissolved metal. A further addition of this metal will then have no effect on the freezing point of the solution; we shall have reached the eutectic state. The freezing point of such a mixture is as steady as that of a pure substance; the whole mass solidifies at a perfectly constant temperature, and if there is no excess of either metal the resulting solid will be remarkably homogeneous. Many cases of this kind have been studied.

If we plot horizontally the percentage of the dissolved metal B, and the depression of the freezing point be plotted vertically downwards, we can by adding weighed quantities of B to the mixture, and determining the freezing point after each addition, get a series of freezing points which will lie on the broken line L M N (fig. 3). The

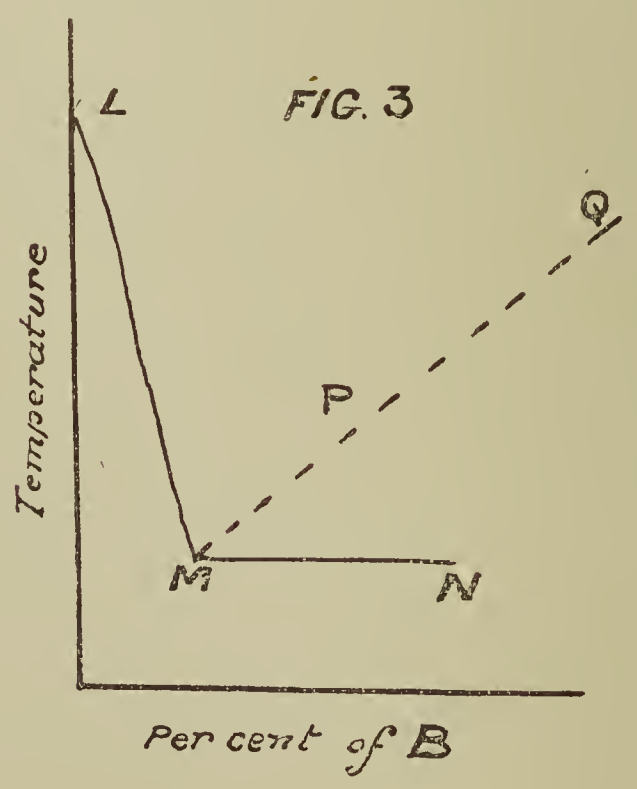


FIG. 3

point L is the freezing point of the pure solvent A, and the horizontal line M N gives the constant freezing point of the eutectic alloy, a temperature which is independent of the excess of B that may be present. If B is sparingly soluble in A the line L M will be very nearly

straight, and its exact solubility at the eutectic temperature can be determined by producing the lines L M and N M to cut each other.

If the solubility of B in A decreases fairly rapidly as the temperature falls, another series of freezing points exists lying on the dotted line M P Q. These indicate the temperature at which the cooling alloy becomes saturated with B, and consequently either B begins to separate or the liquid splits into conjugates. Heycock and the writer[1] have lately succeeded in tracing this line of freezing points; but except when B is present in large excess there is very little solid matter formed at them, and the indication of the thermometer is of a most fugitive character. This phenomenon is not to be confounded with a real rise in the freezing point of the whole alloy, such as that caused by adding silver to cadmium.

The osmotic theory of solution, due to Van't Hoff, leads to the conclusion that $\delta = \frac{1{\cdot}98T^2}{M\lambda}$, where δ is the depression in the freezing point of M grams of the solvent due to the solution in it of one gram-molecular weight of any substance, λ being the latent heat of fusion of the solvent and T its freezing point reckoned from the thermodynamic zero (-273° C.).

If we take as solvent 100 atomic weights of one of the metals in the table below, then δ, the depression predicted by the theory as due to one molecular weight of dissolved body, is given by the numbers in the table in degrees centigrade.

Tin	Bismuth	Cadmium	Lead	Zinc
3° C.	2°	4·5°	6·5°	5·2°

It has been found by Heycock and Neville[2] and by Tammann[3] that in a large number of cases these depressions are produced by the addition of one *atomic weight* of

[1] We hope shortly to publish the experiments bearing on this point.

[2] *Chem. Soc. Jour.*, lvii., 376, 1890; and lix., 888, 1892.

[3] *Zeitschrift Phys. Ch.*, bd. iii., 441, 1889.

dissolved metal. It follows, if the theory can be trusted, that the molecule of a metal when in dilute solution consists of *one atom*. There are numerous exceptions to this rule; for example, aluminium when dissolved in tin appears to form diatomic molecules, and the same is true for zinc palladium and mercury when one of these metals is dissolved in cadmium.

But the nature of the solvent has certainly an influence, over and above that attributed to it by the above formula, in determining the depression; and from what follows it will be seen that the aggregation of the dissolved body into more complex molecules is not the only possible explanation of these abnormally small depressions.

In a few cases the dissolved metal, instead of causing a depression in the freezing point of the solvent, produces a rise. Antimony dissolved in either tin or bismuth, and silver in cadmium or in zinc, produce this effect.

In these cases of a rise in the freezing point experiment has shown that the portion of the mass which solidifies first, instead of being the pure solvent, is richer in the dissolved metal than either the original liquid or the still fluid portion. Van't Hoff[1] has given a theory which fully accounts for this phenomenon. He shows that whenever the matter which separates first contains both metals in the form of a homogeneous mixture, or, as he terms it, a "solid solution," there will either be a diminished depression or a rise in the freezing point. It is worthy of note that in these cases we observe, not the formation of a small amount of solid, but a real rise in the temperature at which the mass of the alloy solidifies and sets in the solid form, even when less than 1 per cent. of the second metal has been added.

If we exclude for the moment cases in which the two metals are combining chemically, and these cases do not seem to be extremely common, it follows from what has been said, that a mixture of two metals in a completely molten state is either homogeneous or is a mechanical mixture of two conjugate liquids, each of which may be

[1] *Zeitschrift für Physikalische Chemie*, bd. v., p. 332.

called a saturated solution of one metal in the other. The proportions in which the two metals were mixed will determine the relative quantities of the two resulting liquids, but will not affect their percentage composition. When the temperature falls the two conjugates will alter in composition, and when solids begin to separate we may expect to find considerable complexity in the resulting body without the necessity of assuming chemical combination to have taken place. Let us consider the case of the metal A, having a high melting point and present in such excess that the second metal B, with a lower melting point, is completely soluble in it. Moreover, let the mixture be above the temperature L at which A melts. If we now slowly cool this homogeneous system one of two things will happen, it may separate into conjugates or it may remain homogeneous until pure A begins to separate out in the solid form at a temperature M below the freezing point L of the pure substance A.

We will consider the latter case. If a thermometer be immersed in the mixture, it will indicate a short period of stationary temperature as soon as A begins to crystallise out; this is the first freezing point. As A continues to separate the solution of B in A becomes stronger, and, in virtue of the Raoult law, A will crystallise out at a continually lower and lower temperature. But a moment must come when, on account of the removal of A in the solid form, the remaining liquid becomes saturated with B. The conjugate of this liquid will now begin to form, and as A continues to freeze out, more and more of this new liquid will be produced at the expense of the first; but during this stage neither liquid will alter in composition, and therefore we should expect A to continue separating at a constant temperature N. As soon as the new saturated solution of A in B is the only liquid left, the temperature will begin to fall with continued separation of A or perhaps of solid conjugates of the remaining liquid. This fall in temperature will continue until the liquid is saturated both with A and B, that is until we have reached the eutectic solution of A in B at the temperature P. From this point A and

B will separate together as solids, or very probably as a solid solution, and the body will remain at a very constant temperature until it is completely solidified. Figure 4 gives

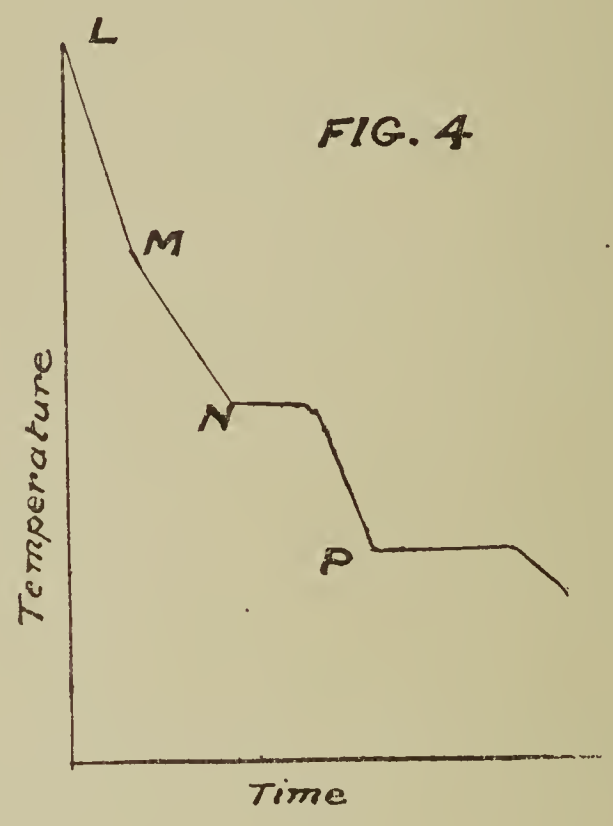


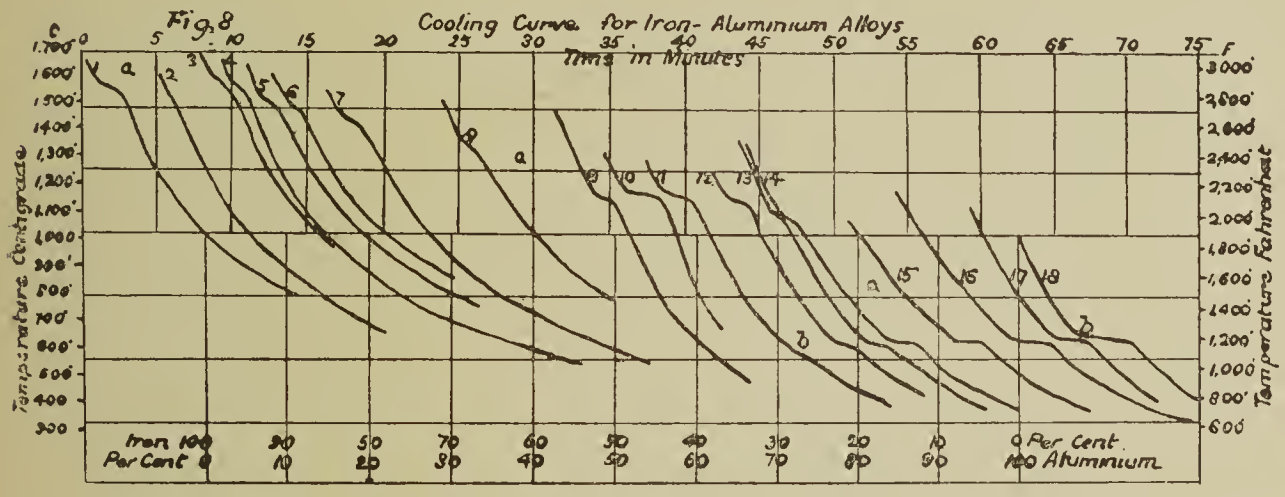
FIG. 4

a diagrammatic representation of these stages in the cooling, temperatures being measured vertically and times horizontally from left to right. Whenever there is a decrease in the rate of cooling due to the separation of solid matter, we shall see in the cooling curve an angle as at M, or if an isothermal transformation is taking place as at N and P there will be a flat.

It is obvious that in an alloy containing but little B, the point M will be well marked because a large amount of A can solidify before the concentration, and therefore the freezing point alters perceptibly; in fact in very dilute solutions there may be a flat at M. With a stronger solution of B the point M is harder to detect and may be missed altogether. The length of the two flats at N and P will depend on the proportions and on the solubilities of the two metals. The temperature M will depend on the amount of B present. The temperature at P, in the eutectic state, is quite independent of the original proportions of the two metals, and, in the case of one or two pairs of metals known to the writer, this is true for the transformation which begins at N. But it must be confessed that, in most

of the binary alloys that have been examined, the temperature from N onwards does depend somewhat on the original proportions of the two metals, and that this stage does not appear to be altogether isothermal. An impurity would produce this effect, but it may be that some other cause is at work.

It would probably require a very delicate thermoscope to detect all these details during the cooling of one alloy, but the whole series is well shown in the cooling curves of iron-aluminium alloys (fig. 5). This figure is taken from



Professor Roberts Austen's "Third Report on Alloys".[1] The curves were obtained by an automatic method of observation. A thermo-junction was placed in the centre of the mass of molten alloy, and a galvanometer in the circuit projected a spot of light on to a sensitive plate. The plate possessed a uniform motion of translation at right angles to that of the spot of light from the mirror of the galvanometer. In this way, as the cooling progressed and the electromotive force of the thermo-couple, and consequently the current passing through the galvanometer, decreased, curves were traced out in which the rate of cooling is indicated by the slope of the curve. Each curve in the figure belongs to an alloy of different composition, the point where each curve cuts the line of 1000° giving the percentage composition, which can be read off from the scale at the base of the figure.

It will be seen that all the curves approach the horizontal at low temperatures. This is due to the fact that

[1] *Engineering*, 1895.

the rate of cooling of any substance is necessarily greater, the greater the difference in temperature between it and surrounding bodies, and this feature in the curves has no special meaning.

On examining these curves we see that the upper part of numbers 1 to 7 contains a well-marked flat due to the Raoult effect, and that this becomes fainter as the percentage of aluminium increases. From 9 to 12 we see a series of flats occurring at a nearly constant temperature, corresponding probably to the formation of conjugate liquids, while from 12 onwards in the alloys rich in aluminium we see a well-marked flat rather below the melting point of aluminium (654°). This occurs at exactly the same temperature in each case, and is certainly the eutectic flat for aluminium saturated with iron. The copper-tin and copper-bismuth curves given by Roberts Austen show similar features.

If the preceding analysis of the process of solidification be correct, then during the whole of the period from M to P (fig. 4) crystals of A or of a solid solution of B in A will be separating out and forming nuclei of solid matter, while from P onwards a liquid of constant composition will be solidifying in the spaces between the crystals already formed. This liquid which solidifies last is independent of the original percentage composition of the alloy, so that there is some danger of mistaking it for a definite compound. A structure corresponding to the above has been observed in the microscopical examination of alloys.

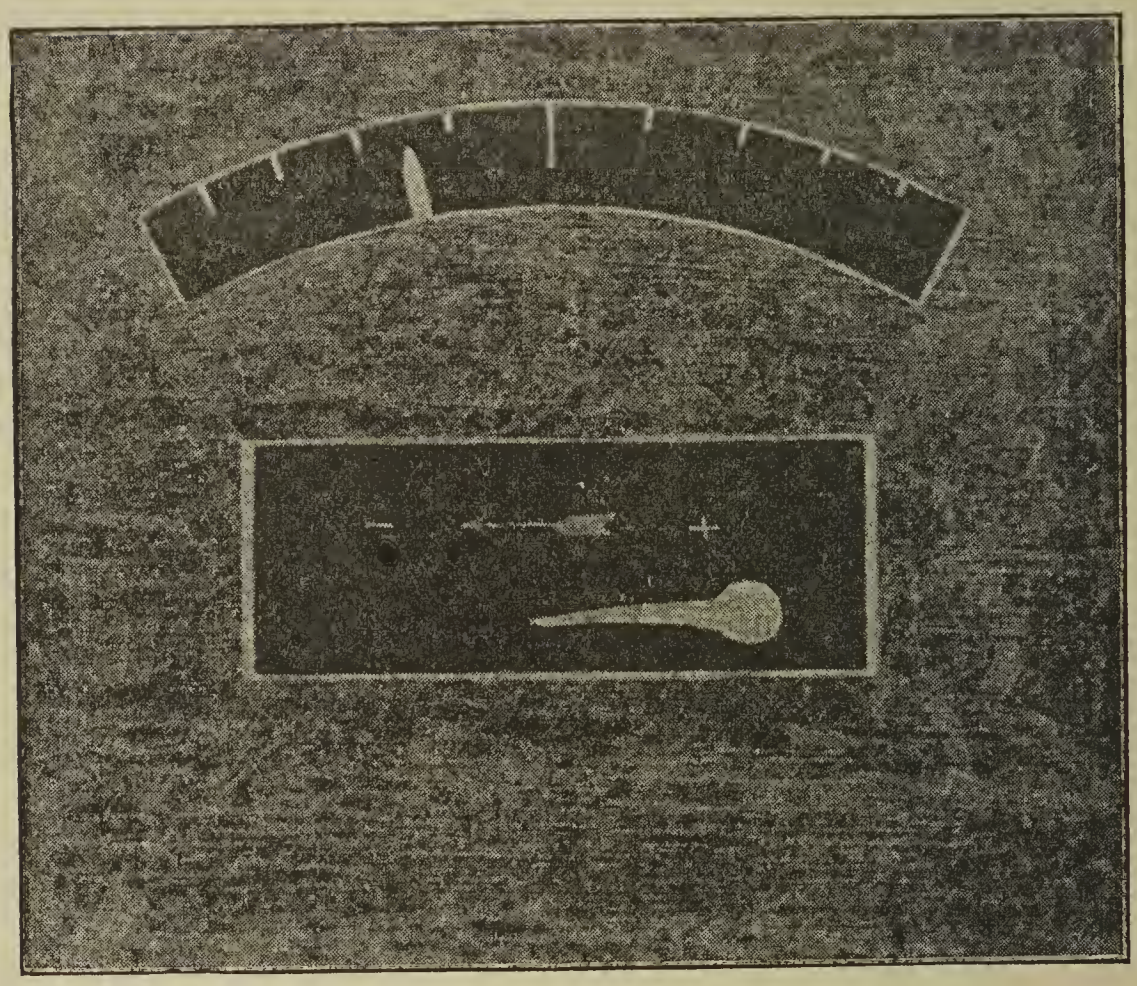
The cooling curve may be much modified in special cases. For example, it is conceivable that the freezing point of the eutectic solution of A in B might be above N instead of below it. In this case the flat at N will be prolonged, and the second flat will disappear. The same change in the diagram, though from different causes, will occur if the two metals are miscible in all proportions, like lead and tin or zinc and cadmium. Surfusion may sometimes cause a rise at the point M, and the formation of chemical compounds might profoundly alter the curve.

It appears probable that mechanical strength will largely

depend on the expansions or contractions that take place during the final solidification of the lower eutectic alloy. Behrens has found by microscopical examination that this most fusible alloy can sometimes be seen to have contracted in freezing, so that minute spaces are left between the less fusible crystals that form the bulk of the substance. Here we have a source of great weakness in a material that, to other than a microscopical examination, would appear homogeneous. Lead-gold and bismuth-gold alloys are probably examples of the same phenomenon. A very little lead makes gold brittle, because of the great contraction of the lead during the final stage of freezing. Perhaps the brittleness of a gold-bismuth alloy may be due to the considerable expansion which is known to take place during this final stage, a state of strain being thereby produced in the substance.

F. H. Neville.

(*To be continued.*)



GALVANOTROPISM OF TADPOLES.[1]

GENTLEMEN, you have before you on the screen an image of a tadpole in a rectangular trough of water through which a galvanic current will be passed *via* two flat metallic surfaces forming the two short ends of the trough. Just above is the image of a galvanometric pointer, the movements of which will indicate the direction and strength of current. A commutator in circuit varies the direction of current, and a resistance that can be altered by turning a screw (*i.e.*, an Engelmann's screw-rheostat) serves to vary its strength. The curved scale projected with the pointer upon the screen is graduated in milli-ampères.

It is not enough to say that so many milliampères of current are used, we must also—in order to have some idea of what passes through the tadpole—know what is the "current-density" in the water. Obviously a current of say one milliampère will be of less density if it is passing through a wide than through a narrow column of water

[1] A Lecture in Electro-Physiology delivered by Dr. Augustus Waller, M.D., F.R.S., at his private laboratory, in June, 1895.—EDITOR.

with the same current-strength, through columns of 10 cm.^{2} and 100 cm.^{2} sectional area; the current-density in the first case is ten times what it is in the second. And the tadpole accordingly is traversed by more current in the first than in the second case.

To know where we are we must therefore agree upon some unit of current-density, and ascertain from measurement of the sectional area of the water column how many such units are passed through it, and—assuming the resistance of the tadpole to be equal to that of the water[1]—through the tadpole.

We shall take as the most convenient unit of this order the density afforded by 1 ampère through a column of 1 square metre sectional area, a value that we may denote by the symbol 1*a*.

You will best realise the notion of this unit by tabulating a few values:—

1 ampère through	1 square metre has the density	1*a*
1	0·5	2*a*
1	0·01	100*a*
1	1 square centimetre,	10,000*a*
1 milliampère,	1	10*a*
1	10	1*a*
1	5	2*a*
2	5	4*a*
etc.,		etc.

The circumference of my wrist is 17 cm., *i.e.*, the transverse section is about 23 cm.^{2} If I stand say a current of 5 milliampères this implies an average current-density of $\frac{5}{23}$ milliampères per cm.^{2} $\frac{5 \times 10{,}000}{23}$ milliampères, *i.e.*, 2000 milliampères per square metre, *viz.*, 2*a*.

[1] The resistances are not equal, but this will not affect the dosage, which will be correctly enough expressed by the number of density units through the water. If the tadpole is in a fluid of lower resistance than itself it will receive less than its share of current, if in a fluid of higher resistance more than its share of current. This is easily verified by testing a tadpole in normal saline; when a far larger current is required to bring off the effects described.

The sectional area of the water in this trough has been made as nearly as possible 5 cm.^{2}, and since each division of the scale is 1 milliampère it follows that each half division indicates a current-density through the water (and through the tadpole) of 1*a*.

We may now proceed with an experiment. The tadpole is at rest; closing the circuit I pass through it a current of 1 milliampère (*i.e.*, of 2*a*), the animal is agitated and turns its head to anode, since its head is to the anode and its agitation takes the form of swimming movements, it swims to the anode and there stays quiet. I now reverse the current, the animal is agitated, turns round and swims across the trough to the new anode. I reverse again and the tadpole passes over again, always moving oppositely to the pointer, *i.e.*, against the current from kathode to anode.

This, however, has not been a perfectly typical experiment in so far as it has not clearly exhibited what—in agreement with Hermann who first made the observations, and in disagreement with Ewald who contradicted them—I have been led to admit as the fundamental fact.

It is not always possible to predict the direction in which a given tadpole will swim, nor that which it will finally occupy; it may and often does swim the other way, *i.e.*, towards the kathode, and there remain more or less quiescent as long as the current is passing. But if following Hermann's plan we observe a number of tadpoles in a large trough (Hermann's trough was 180 × 63 mm.) traversed by a moderately weak current (1 to 2*a*), we shall be able to see that a large number of tadpoles point motionless towards the anode, that others swim towards the anode, and that a few point and swim towards the kathode.

Here is what I find to be a "safe" and convenient form of experiment: two tadpoles are placed in a smaller trough (90 × 30 mm.) in the lantern-field, with a current of about 4*a*, alternated in direction two or three times; their, for this purpose, somewhat too exuberant vivacity is toned down, so that they no longer dart about upon the smallest provocation. They are somewhat sluggish, their move-

ments are, so to speak, more dead beat, and, in the absence of current, or even while the current is passing, the position of the long axis remains stable in any position, preferably antidrome, *i.e.*, with head to anode, but frequently homodrome, with head to kathode. In the antidrome position, as you may see, they are perfectly still as if paralysed, in the homodrome position they wag their tails. I wake them up by reversing the current; after a dart here and there they settle again and it may be in any position. Both may look up stream or down stream, or one may look one way, the other the other. And now, to quickly realise the relation between direction of current and position of tadpole, I am sure from my own hesitation, in spite of the pointer showing how the current is going, that you do not instantly recognise the sudden changes under the correct rubric, "homodrome," "antidrome". So let me use domestic language, and, comparing our tadpoles with cats, speak of the current as stroking them the right way, *viz.*, from head to tail, or stroking them the wrong way, from tail to head.

You will, I think, readily admit a valid excuse for such untechnical language when you have recognised by a rapid series of trials that stroking the right way pacifies, stroking the wrong way excites, *i.e.*, in technical language, arrest by an antidrome current, excitation by a homodrome current. The tadpoles happen to be lying side by side and I cautiously stroke them the wrong way by gradually putting on current with the screw-rheostat, they both wag their tails. I cautiously reverse the current, unscrewing the rheostat, reversing, then screwing again, guiding myself as to current-strength by the current indicator; the tadpoles, which are now being stroked the right way, lie perfectly still. Or better still, having been stirred up to greater activity by sudden reversal or reversals of current, they have come to rest side by side with their heads in opposite ways; and now I am able to play at see-saw, by cautiously putting on current and cautiously reversing it. Always the tadpole stroked down the wrong way wags his tail, while his companion, who is at the same time being stroked down

the right way, remains perfectly still; and on reversal of the current the behaviour of the two individuals is reversed, *e da capo*.

So that, with Hermann, we may be satisfied to conclude that the essential fact upon which the galvanotropic effect turns is that current down the animal has a quieting action, current up the animal an exciting action.

The presence of the spinal cord is an indispensable condition of the success of the experiment. In this trough I have now placed two tadpole tails, one cut off near the head, the other about half-way down; the first includes a bit of spinal cord, the second does not; both tails lie along the line of current, which, as you see, causes the first tail to tremble when it passes in an upward direction. There is not a tremor to be seen when the current passes in an opposite direction, *i.e.*, towards the tip. The other tail, *viz.*, that without spinal cord, remains motionless with both directions of current.

We are now properly prepared to see more clearly than would otherwise have been the case the *rationale* of the experiment in its first and most striking form, as first witnessed and described by Hermann.

This flat trough (100 × 75 mm.), about as large as will fit the lantern, contains a number of fresh tadpoles, moving more or less leisurely and jostling each other in all directions. I suddenly close a key, sending through the water and the tadpoles a current of about 3*a*. The commotion is amazing, the tadpole community seems to have gone mad, a writhing mass is all that can be distinguished; but the disturbance does not take long to subside, and now all the tadpoles are fixed as if at attention looking one way, heads to anode, *viz.*, traversed by a current from head to tail, stroked down the right way. This is galvanotropism in so far as it is an orientation of the body under the influence of the galvanic current, but it is hardly galvanotropism of a simple and fundamental character analogous with the galvanotropism of protozoa, or the heliotropism of plants. It is a reflex adjustment of the animal to its environment effected through the agency of the nervous system; the tadpoles have ex-

perienced excitation in all positions of the body but one; the position of least excitation, or we must even say the most soothing position, is enforced upon them by that experience. Stroking the wrong way excites them, they therefore avoid the positions in which that happens. Stroking the right way pacifies them, and they therefore adopt the positions in which that is best received.

GALVANOTROPISM.

In the lowest forms of animal life—amœbæ, infusoria, etc.—the polar response to excitation by the constant current is diametrically opposed to that obtaining upon differentiated muscle and nerve. Whereas upon the latter the make excitation is kathodic, and the break excitation anodic, upon non-fibrillated protoplasm the make excitation is anodic and the break excitation kathodic. With these effects there is in many cases a remarkable orientation in relation to the current—"galvanotropism"—accompanied in some instances by an evident disintegration of protoplasm on the anodic side (*e.g.*, in Actinosphœrium and in Pelomyxa), in others (*e.g.*, in Paramœcium), by amœboid or swimming movements in the direction of the current, *i.e.*, away from the anode. In general the orientation of Infusoria is such that they arrange themselves longitudinally in the lines of current with their anterior ends towards the kathode, and their swimming movements, if any, in that direction (*e.g.*, Paramœcium); in some instances (*e.g.*, Polytoma uvella, Cryptomonas erosa), the orientation is reversed; in others again it is transverse, the long axis of the animal disposing itself at right angles to the current (Verworn).

Fertilised frog's eggs exhibit according to Roux very remarkable galvanotropic effects; in a layer of spawn traversed by strong alternating currents (100 volts), each individual egg presents at the end of a few minutes a division into three zones—two dark polar areæ separated by a light equatorial zone—and the eggs collectively dispose themselves in conformity with the lines of current-diffusion so that the light equatorial stripes lie at right angles to the lines

of current, thus indicating the general direction of equipotential curves (which we know to be at right angles to the lines of current).

A curious sight is offered by the behaviour of a drop of hay-infusion, containing paramœcia in sufficient abundance, placed in a flat vessel into which dip the unpolarisable electrodes of a battery of 2 to 5 volts. At make the whole crowd of paramœcia fall into order with their noses towards the kathode, and begin to swim towards it in converging curves ; in a few minutes they are thickly crowded round the kathode, and the fluid round the anode is quite cleared ; if now the direction of the current is reversed, the crowd breaks up, all its units turn round and begin to swim away as if of one mind from the new anode to the new kathode, to which the crowd converges as before.

There does not seem to be any unanimity among the protozoa as to the direction they shall adopt in relation to the current. Paramœcia swim with the current from anode to kathode ; in this little electric bath you can follow them with the naked eye, but I will throw them upon the screen, when you will see them better still. As I put on the current they turn with one accord to the kathode and swim to the kathodic bank of their lake, as I reverse the current they turn round and swim over to the other bank ; there happen to be not very many paramœcia in this infusion. What is happening on the screen is absurdly like what one may see in the country ; these little brown spots swimming across their lake from bank to bank look just like rabbits scampering across a field from one cover to another, and as we reverse the current they scurry across backwards and forwards as if beaten out of cover as soon as they have found it. If one reverses the current at a moment when they are scurrying across at the top of their speed, they turn themselves and rush off the other way.

Ciliated infusoria, as a rule, agree with paramœcia and swim with the current. The custom among flagellata, on the other hand, is to swim the other way ; they affect the anode or the anode affects them. It is difficult to resist laughing when one has under one's eyes the spectacle of a

galvanic bath such as this, containing a mixture of ciliated and flagellated protozoa. In the absence of any electrical current they are swimming about in all directions, intermingling in the most friendly manner. Make the current and two armies, so to speak, assemble themselves on the two banks ; ciliata to the kathode, flagellata to the anode! appears to have been their *mot d' ordre ;* and now if you reverse the current the two armies put themselves in motion and appear as if precipitating themselves upon each other, but no disaster happens, the opposing crowds slip through each other and reassemble at the pole which they respectively affect.

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AUGUSTUS WALLER.

THE CHROMATOPHORES OF ANIMALS.

THE wide distribution of pigments throughout the tissues of animals, the differences in origin, nature, and function which they present, and the variety of forms under which they are manifested, render the exact definition of the term chromatophore in the present condition of knowledge a matter of some difficulty. The term has indeed been restricted by Pouchet to the pigment bodies of Cephalopods, and a distinction has been drawn by him between these structures and the pigment-cells of Vertebrates, which he has termed chromatoblasts. As it is now, however, indisputable that the pigment bodies of Cephalopods are true cells, it is no longer possible to restrict the term in this way: chromatophore and chromatoblast are synonymous.

Chromatophores may safely be defined as pigmented cells; but all pigmented cells are not chromatophores. We have to distinguish the essentially chromatic cell from other pigmented cells whose colour, as such, is immaterial. The question is one of limitation: which pigmented cells are chromatophores? It would be meaningless, for example, to apply the term chromatophore to pigmented cells of the excretory, or respiratory, or sensory tissues. By following this principle of elimination the physiological answer to the question becomes apparent: chromatophores are pigmented cells whose essential function is chromatic, that is, to bestow particular colours upon an animal. In the case of respiratory or nutritive pigmented cells the colour is unessential, and is a mere attribute of the particular respiratory or nutritive bodies, as blue is an attribute of copper sulphate; but in the case of chromatophores the colour is all in all, and the nature of the pigmented bodies altogether immaterial.

The difficulty is to draw the line. In the cuttle-fish or frog the chromatophores are easily recognisable owing to their highly specialised form and their readily demonstrable function. But what are we to do in those numerous cases

of highly coloured animals in which the value of colour is still hypothetical, and in which there is no peculiarity of form to distinguish any purely chromatic cells from other pigmented cells which may be inferred to discharge respiratory, nutritive or other functions? The Turbellaria and Echinoidea afford cases in point.

Two attitudes are possible. The term chromatophore may be applied to all pigmented cells which have not been shown to discharge a respiratory or other non-chromatic function; or it may be restricted to pigmented cells which are without doubt chromatic in function. The latter attitude seems to me preferable, since it begs no question and yet conduces to precision of language.

Chromatophores are thus pigmented cells specialised for the discharge of the chromatic function.

This definition evades the difficulty presented by pigmented cells which, although respiratory or excretory, etc., in function, do, nevertheless, contribute to the normal coloration of the animal, as, for example, the elements of the vaso-fibrous tissue of leeches. There still remains the question whether the term chromatophore is applicable to the cells of an epithelium in which pigment is deposited for indubitably chromatic purposes, but which are not distinguished by any other features from the unpigmented elements of the same tissue. This case is presented by the Insecta and Brachyura, in which groups the colours of the pigments deposited in ordinary cells of the epidermis (hypodermis of authors) frequently assume a high protective significance. It is far from improbable that the distinctly specialised chromatophores of other Arthropods have been developed from such unspecialised pigmented cells as those of the Insectan epidermis, so that the latter may certainly be regarded as incipient chromatophores; but the fact that they are in no way set apart for chromatic purposes, that this function is merely superadded to their ordinary task of secreting the protective cuticle of the body, seems to me to be decisive against their claim to the title chromatophore in any other than a purely etymological sense.

With the ground thus cleared we may proceed to the

more immediate object of the present paper, namely, to reveal what light has been thrown by increase of research upon the evolution and mechanism of true chromatophores.

Since it is evident that chromatophores are simply pigmented cells specialised for a particular purpose, the problem of their evolution cannot be adequately considered except by the comparative method, which in this case involves a survey of pigment-cells in general.

In this survey special attention has been paid to two points :—

i. The localisation of the pigment-cells and their relation to the germinal layers.

ii. The nature of the structural modifications exhibited by pigment-cells.

I have neglected all references to intercellular pigmentation, since this phenomenon, important as it is in regard to the history of pigment-cells in general, has no particular bearings upon the origin of chromatophores.

1. *Porifera.* Pigment-cells of oval, spindle-shaped, or stellate forms are described by Schulze and Vosmaer for various non-calcareous sponges. Their origin appears to be unknown (Vosmaer, *Bronn's Klassen und Ordnungen*, ii., 1887).

2. *Cœlentera.* In the Hydrozoa pigment may be deposited either in the ectoderm or in the endoderm. The varied colours of the Hydroidea, especially the Gymnoblastea, are almost entirely due to pigmentation of the endodermal cells (Allman), but ectodermal pigment is found in the ocelli of Craspedote Medusæ and occasionally in the epithelium covering the generative glands (Hertwig).

The brilliant colours of many Acraspedote Medusæ appear to be chiefly due to pigments deposited in the ectoderm (Haeckel).

In the Anthozoa the brilliant colours are chiefly due to so-called zooxanthellæ, which have been interpreted by the Hertwigs as symbiotic algæ. The earlier observers (Haime, M. Edwards, Gosse) described a special pigmented layer of the ectoderm; but Heider has failed to find any special pigment-cells, and he attributes the

markings of the oral disc in *Sagartia troglodytes* to the distribution of the various coloured ectodermal gland-cells (*Sitzungsberichte Akad. Wiss. Wien*, lxxv., p. 394, 1877). The brown colour of the column and base of *Sagartia parasitica* is caused, according to the Hertwigs, by the pigmentation of "connective tissue cells" within the mesoglœa (*Die Actinien*, p. 45, 1879).

The highest development of the pigment-cell in Cœlentera is found in the Ctenophora, and in this group the pigment-cells are exclusively ectodermal. Apart from the "glance-cells" and fluorescent cells of *Cestus Veneris*, the former of which seem to foreshadow the iridocytes of higher forms, rose-coloured pigment-cells are found in *Callianira*, and beautiful chromatophores, yellow, brownish-red, or rose in colour, deck the external epithelium of various species of *Beroë* and of *Euchlora rubra*. The chromatophores of *Beroë* are stellate, and contract upon stimulation; they are developed from simple unbranched cells of the ectoderm (Chun).

3. *Turbellaria.* In Rhabdocoelida the body colours are seated as a rule in the sub-epidermic parenchyme (L. von Graff). The pigments may be either (1) granular (the usual type) and distributed in connective tissue cells, *e.g.*, most Acœla, or in fibres of the parenchyme, forming a reticulum; or (2) in solution, either (*a*) diffused throughout the protoplasm of the cell, or (*b*) restricted to one or several intracellular vacuoles, *e.g.*, various species of *Vortex*.

When the pigment is ectodermal it may be either (1) granular, (2) in solution and diffused through the cell-body, or (3) in the form of coloured rods, *e.g.*, *Convoluta flavibacillum*.

Among Dendrocoelida the colour of the body in the suckerless Polyclads (Acotylea) may be largely due to curious granular vesicles of unknown significance lying in the ectoderm (Lang, *Die Polycladen*, 1884).

In the sucker-bearing Polyclads (Cotylea) pigment is deposited either in vacuolised cells of the ectodermal epithelium or in non-vacuolised cells of the mesodermal parenchyme. In the former case the pigment is mostly yellow,

orange or red, and consists either of coloured fluid within the vacuole alone, or of granules found in the peripheral protoplasmic portion of the cell and subsequently dehisced into the fluid contents of the enlarging central vacuole. The mesodermal pigment is in the form of black or brown granules deposited in the peripheral portions of the cells. The two kinds of pigment—bright ectodermal and dark mesodermal—may exist separately or together, and the general colour of the Planarian depends upon the preponderance of one or other of the different elements.

4. *Nemertea.* The brilliant colours of these animals are said by M'Intosh and Hubrecht to be due to pigmented cells of the connective tissue. Bürger, however, has shown (*Zeit. f. wiss. Zool.*, l., 1890) that in the most primitive Nemertines, *e.g.*, *Carinella*, the pigment resides, not in elements of the connective tissue, but in the non-glandular elements of the ectoderm. In all higher forms the pigment is contained in branched pigment-cells of the ordinary type, which are situated either entirely beneath the basement membrane of the ectodermal epithelium, as in Metanemertines, or partly above and partly below this membrane, as in *Eupolia.* In the latter case the pigment-cells are associated with the enlarged gland-cells of the ectoderm—a fact which suggests their derivation from the same embryonic layer.

5. *Hirudinea.* The pigment is entirely mesodermal, and is extensively deposited in cells of the connective and vasifactive tissues. The pigment found in the spaces between the mallet-shaped cells of the epidermis has intruded from the connective tissues (Lankester and A. G. Bourne).

6. *Annelida.* The bright orange colour of the Archiannelid *Dinophilus tæniatus* is due to pigment contained in large branched connective tissue cells. Orange-coloured granules are also contained in the cells of the nephridia (Harmer, *Jour., M. B. A.*, i., 1889).

According to Claparède (*Annélides Chétopodes du Golfe de Naples*, p. 158) the colours of Polychæte Annelids are generally due to pigment distributed in the chitinogenous, that is ectodermal, layer. The pigmented ectodermal cells

of *Hermione hystrix* differ, according to Jourdan (*Arch. Zool. Exp.*, v., p. 94, 1887), from the polygonal unpigmented cells of the same layer by their smaller size. Where the epithelium is columnar, as in *Telepsavus*, the pigment is contained in the outer portions of the cells (Claparède, *Ann. Sed.*, pl. xiii., figs. 9 and 19, 1873).

Pigment may also, however, be extensively deposited in the cells of the peritoneum, as in *Nereis peritonealis* and *perivisceralis* (Claparède). In the former case it is violet, in the latter brown in colour. "Chloragogen" cells—enlarged pigmentiferous cells of the splanchnopleure—are also widely distributed among Annelids. The blood in many Annelids also contains special highly coloured respiratory pigments.

Much-branched pigment-cells of a yellow or reddish-brown colour are found in certain otherwise transparent pelagic Polychætes, such as *Pontodora pelagica*, *Phalacrophorus pictus* (Greeff, *Zeit. f. w. Zool.*, xxxii., p. 237, 1879), and *Iopsilus phalacroides* (Viguier, *Arch. Zool. Exp.* (2), iv., 1886). They appear to lie in the connective tissue, and seem to have a certain relation with the nervous system. In form and arrangement they foreshadow the chromatophores of Crustacea.

In Oligochæta the pigments are generally mesodermal. Interesting exceptions to this rule are furnished by various species of *Æolosoma* and *Ctenodrilus*, in which a number of ectodermal cells scattered over the body are known to contain large oily drops, generally coloured in various brilliant tints of green (Von Kennel, *Arb. Zool. Inst. Wurzburg*, v., 1882 ; Beddard, *Proc. Zool. Soc.*, 1888).

7. *Gephyrea.* Respiratory and excretory pigments are common. The dermal layer of the skin of *Sipunculus nudus* contains numerous pigment aggregations, apparently cellular, the precise nature of which cannot be said to have been satisfactorily ascertained (Andreæ, *Zeit. f. w. Zool.*, xxxvi., 1882). It is interesting to note that in the larva somewhat similar bodies are found within the ectoderm. These are stated to disappear during the metamorphosis of the larva (Hatschek, *Wien Arbeiten*, v., 1884); but the

question arises as to whether they may not have some connection with the dermal deposits of the adult. Balls of pigmented cells also occur in the sub-epidermal tissues of Echiurids (Spengel, *Zeit. f. w. Zool.*, xxxiv., 1880).

8. *Crustacea.* Pigments may be deposited either in the ectodermal epithelium or in special highly branched sub-epithelial cells, usually referred to the connective tissue and known as chromatophores. The latter are chiefly found in Malacostraca, but are also known to occur among Ostracoda in various species of *Loxoconcha* (G. W. Müller, 1884). Crustacean chromatophores are usually highly mobile, and in Isopods, at any rate, the chromatophore is contractile as a whole (Matzdorff, *Jena Zeit.*, 1883). The presence of chromatophores is usually associated with transparency of the body, *e.g.*, Amphipoda, Schizopoda, Prawns; epithelial pigmentation naturally entails opacity, as in the larger Macrura and most Brachyura.

9. *Tracheata.* In *Peripatus* the characteristic colours of the body are due to pigment granules deposited in the protoplasm of the outer ends of the ectodermal cells (Balfour, *Q. J. M. S.*, 1883).

The same is true of Insects in general (Poulton, *Proc. Roy. Soc.*, xxxviii., 1885; *Trans. Ent. Soc.*, pl. x., 1887); and probably also of Arachnids. But the branched pigment-cells in the central eyes of Scorpions (and *Limulus*) and in the compound eyes of Insects are believed by Lankester and Bourne to be intrusive connective tissue cells (*Q. J. M. S.*, xxiii., 1883).

10. *Mollusca.* The integument is brilliantly coloured in certain Amphineura, *e.g.*, *Dondersia banyulensis*, but the exact localisation of the pigment is not described (Pruvot, *Arch. Zool. Exp.* (2), ix., 1891).

In Gastropoda the pigment occurs either in the ectodermal epithelium, as in *Haliotis*, *Neritina*, Heteropoda, *Doris;* or in the sub-epithelial connective tissue (Boll, *Arch. f. mikr. Anat.*, v., *Suppl.*, p. 51, 1869). In the latter case the pigment may reside in true chromatophores, as in Pteropods and Cephalopods, or in great glandular cells lying in the connective tissue but communicating with

the exterior by a narrow neck, and belonging really to the ectodermal layer (Leydig, *Arch. f. Naturg.*, xlii., 1876; Boll, *l. c.*). This type of pigmentation occurs in Pulmonata; but there is also in this group, just beneath the epithelium, a dense pigmented network, which consists, according to Leydig, of branched mobile pigment-cells.

11. *Echinoderma.* In Holothurians intracellular pigment may be restricted to a granular deposit in the ectodermal epithelium, *e.g.*, in *Stichopus regalis*, or may occur in the form of bead-like deposits along thread-like prolongations of the basal ends of ectodermal cells (Jourdain, *Ann. Mus. Hist. Nat.*, Marseille, i., 1883), or in the terminal fibres of the cutaneous nerve-plexus. Ludwig also mentions the existence of richly branched pigment-cells (*Bronn's Thier-Reichs*).

In Asterids the brilliant red and violet pigments are in the form of granules deposited in the cells of the external epithelium (Cuénot, *Arch. Zool. Exp.* (2), v., *bis* 1887-1890). The epidermal pigment-cells of the optic pits are prolonged at their bases into long threads enclosing bead-like deposits of pigment as in Holothurians (Hamann, *Beiträge*, Asteriden, Jena, pp. 17-19, 1885).

In Echinids the pigment is contained in branched contractile cells, which lie either in the connective tissue layer, in the space between the epidermis and the muscle layer, or in the epidermis itself. Hamann interprets these cells as wandering cells of mesodermal origin (*Beiträge*, Echiniden, pp. 22, 30, 43, 45, 53, 85, 1887).

12. *Tunicata.* Pigment is almost entirely restricted to the blood-corpuscles and other mesodermal elements, which are often vividly and variously coloured, *e.g.*, in *Botryllus.* Mesodermal pigmented cells may penetrate into the cellulose test. Ramified pigment-cells have been observed in *Salpa bicaudata* (Gegenbaur, Kölliker u. Müller, *Zeit. f. w. Zool.*, p. 331, 1853).

13. *Vertebrata.* The branched mobile chromatophores of Vertebrates occur both in the epidermis and the dermis, most frequently in the latter. Their structure is identical in the two cases, and on account of their greater frequency

in the mesodermal tissues and their resemblance in form to various unpigmented cells of the connective tissues the chromatophores both of the epidermal and subjacent layers are usually regarded as being of mesodermal origin.

Pigment is also frequently deposited in the ordinary unbranched cells of the deeper layers of the epidermis (Kölliker, *Handbuch der Gewebelehre*, Leipzig, 1889).

From the above survey it may be seen that although pigment-cells are widely distributed, they attain a high elaboration of form and function in relatively few phyla of the animal kingdom. In its highest form the pigment-cell is mobile and under the influence of the nervous system; but such chromatophores are found only among Vertebrata, among Cephalopod and Pteropod Mollusca, among Crustacea, and possibly in a few pelagic Annelida and Ctenophora. Branched pigment-cells are also found in Porifera, Nemertea, in the compound eyes of various Tracheata, in Pulmonate Mollusca, and in Echinoderms; but in these cases the pigment-cell is apparently incapable of those movements which in the chromatophore of the Cephalopod or Vertebrate cause marked alterations in the colour of the animal. It is even questionable whether the branched form of these pigment-cells is in any way connected with purposes of colour display. It is impossible to maintain that colour in itself is of any value to a sponge; and the pigment-cells of Echinids have clearly not been specialised for chromatic purposes, since the majority of them are situated within the shell where they cannot affect the coloration of the animal. Moreover, in Holothurians pigment-laden wandering cells have been shown to discharge an excretory function (Schultz, *Biol. Centr.*, xv., p. 390, 1895). The function of the pigment-cells in Nemertea and Pulmonata is quite problematic; but a nutritive or excretory function could be assigned to them with just as much, or little, reason as could a chromatic function.

If we now take into consideration the immobile unbranched type of pigment-cell as found among the Turbellaria or the Oligochæta *Æolosoma* and *Ctenodrilus*, we again find that the modifications which these cells exhibit

are not clearly correlated with any chromatic function. The entoplastic modifications shown by these pigment-cells are in no way different from those which characterise ordinary storage cells of reserve or waste materials.

It would thus appear that the only pigment-cells distinctly specialised for chromatic purposes are those of Vertebrata, of Cephalopod and certain Pteropod Mollusca, and of Crustacea. To these pigment-cells the term chromatophore should be at present restricted, though it is very probable that the mobile pigment-cells of certain Ctenophores and the branched pigment-cells of certain pelagic Annelids will eventually find a place in the same category.

We are now in a position to confront the question of the origin of chromatophores. Have these remarkable elements arisen independently of the remaining types of pigmented cells, or have they been formed from pre-existing pigmented cells specialised for chromatic purposes? Also, do chromatophores exhibit any constant relation to the germinal layers?

The answer to these questions can only be given after a separate examination of the history of the chromatophores in each group. Before proceeding to this examination it may be well to point out the following considerations which are not without weight as guides to the formation of an opinion upon the subject.

The usefulness of pigment for chromatic purposes could only begin after the pigment had originated, and it is therefore probable that chromatophores have arisen by the modification of pre-existing pigmented cells. Moreover, since the prime necessity of chromatophores is that they should be visible, it is probable that they would arise in the external layer of the body, the ectoderm. This view is at variance with current opinion, according to which chromatophores are of the nature of connective tissue elements; but I shall show that there are many facts in the history of chromatophores which are quite irreconcilable with this opinion, and that the body of evidence strongly supports the view which I am propounding.

In the first place the branched contractile cells of

Ctenophores are indubitably ectodermal both in origin and position; and I shall show below that the chromatophores of Cephalopods which are mesodermal in position are ectodermal in origin.

In the second place it can be shown that pigment-cells which are usually sub-epidermal in position in certain groups have in all probability migrated inwards from the ectoderm. The Nemertea offer good examples of this phenomenon. In the Protonemertini, *e.g.*, in *Carinella*, which possess no cutis, the pigmented cells form part of the ectodermal epithelium (Bürger, *l. c.*, taf. ix., fig. 123); and the Metanemertini, or Enopla, retain this primitive condition (*l. c.*, p. 178, fig. 127). But the Heteronemertini possess a subepithelial layer or cutis which is not found in the other groups, and branched pigment-cells are contained within this layer. Deeply as the pigment-cells extend in many forms, *e.g.*, *Lineus gesserensis*, they are confined to the external glandular layer of the cutis in the most primitive Heteronemertines, *e.g.*, *Eupolia*, and in *Eupolia Brockii* the pigment-cells are only partially contained within the cutis, their upper extremities stretching between the epithelial cells to the external surface of the epithelium (Bürger, *l. c.*, pp. 33, 50, fig. 26). The series is complete, and there can be scarcely a doubt that the pigment-cells of Heteronemertines are ectodermal elements which have sunk beneath the epithelium *pari passu* with the elongation of the ectodermal glands.

In certain Gephyrea pigment-cells are found within the cutis of the adult; and in the case of *Sipunculus nudus*, at any rate, they are known to be preceded by pigmented bodies in the ectoderm.

Among Crustacea Malacostraca the chromatophores are situated in the mesoderm in the higher forms. But they have been observed in the ectoderm in some Isopods, and just beneath it in others; in the latter case they often possess processes which stretch up through the epithelium to the cuticle above it.

Again, in Echinoderma we have seen that in certain cases the pigment is contained in prolongations of the

ectodermal cells beneath the general layer of the epithelium.

Finally, Kerbert's (33) interesting observations on Vertebrata have shown that during the development of reptiles the chromatophores appear first in the epidermis, and only subsequently in the cutis. In the Lacertinæ a certain number of chromatophores are retained in the epidermis even in the adult; but in snakes, whose scales attain a higher degree of differentiation than is the case with those of lizards, no chromatophores are retained in the epidermis after embryonic life; the epidermic stage of the chromatophores in snakes is transitory.

It would thus appear that there is a considerable amount of evidence in favour of the view that chromatophores in general are of ectodermal origin, whatever be their ultimate position in the body.

The Chromatophores of Crustacea. The literature which bears upon the subject of chromatophores in Crustacea deals chiefly with the sessile-eyed Malacostraca, and the investigations which point to the ectodermal origin of chromatophores deal principally with certain Isopoda.

In *Trichoniscus*, according to Max Weber (1), the ectoderm is a syncytium and consists of nuclei embedded in a granular sheet of protoplasm beneath the chitinous cuticle. Only here and there, for example at the junction of two segments, can a definite epithelium be distinguished. The chromatophores lie partly in the connective tissue of the body cavity, and partly in a layer just beneath the ectoderm. In the latter case, however, although the bodies of the chromatophores may lie beneath the ectoderm, many of their processes are found within the syncytium itself and often extend as far as the under surface of the cuticle. Here and there indeed entire chromatophores are found embedded in the subcuticular syncytium, and their branches, anastomosing with those of neighbouring chromatophores, form a net-work within the syncytium, which often produces the appearance of a regular epithelium, especially when the meshes of the net-work happen each to enclose one of the nuclei of the syncytium. In *Trichoniscus*

Leydigii, which is a true cave animal, branched cells are present in the subcuticular syncytium, which resemble the chromatophores of allied types in all points except that pigment is not deposited within them.

In *Idotea*, according to Matzdorff (2), the chromatophores lie exclusively in the epidermis. This consists of two layers in *Idotea*, an outer epithelium of chitinogenous cells and an inner granular syncytium with regular distributed nuclei but without cell-boundaries. The chromatophores lie in this under layer, and occupy its whole thickness, even bulging slightly into the epithelial layer above and into the connective tissue below. Each chromatophore is a naked nucleated cell without a bounding membrane. Its shape can vary from a spherical or oval to a highly stellate form. The processes bore their way through the protoplasm of the syncytial layer without having any definite paths prescribed for them; and the branches of neighbouring chromatophores can fuse to form an intricate net-work of pigmented tissue. The chromatophores of Isopods are true amœboid cells whose resting condition is that of contraction, and whose active condition is that of expansion.

In Caprellids, according to Paul Mayer (3), the epidermis is cellular, and forms a true pavement epithelium which is one-layered throughout—a statement which he would extend to Arthropoda in general. He agrees with Hoek (4) that the chromatophores are exclusively situated in the connective tissue, and in the envelopes arising from it which surround the gut, nervous system, gonads, and heart.

We thus see that, if the observations of Max Weber and Matzdorff be well founded, the Crustacean chromatophore, though frequently assuming a mesodermal position, is very probably of ectodermal origin. Mayer's generalisation that the epidermis of Arthropods is universally one-layered cannot be absolutely accepted, for it has been shown by Viaillanes (5), Minchin (6), and Mingazzini (7) that in insects at any rate certain branched cells of the epidermis may be found beneath the general level of the epithelium.

I would place the chromatophores of Crustacea in the same category as these branched cells of the Insectan epidermis, and would draw attention to the fact, established by Max Weber, that in cavernicolous forms the chromatophores of the ectoderm are unpigmented, in which condition they closely resemble the branched ectodermal cells of insects.

THE CHROMATOPHORES OF MOLLUSCA. So much attention has been directed to the study of the pigment-cell in Cephalopoda that it is desirable to recall the evidence which exists as to the occurrence of chromatic cells in other groups of the Mollusca.

In *Phyllirhoë* remarkable cells containing a brilliant golden-yellow substance were described in 1854 by H. Müller and Gegenbaur (8). They are grouped in a pair of longitudinal bands, one along the dorsal, and the other along the ventral edge of the body. They are sub-epithelial in position, and their nucleus is easily recognisable in young individuals. They were found to exhibit two modifications of form; one of these so closely resembles that of the chromatophores of Cephalopods that, although no phenomena of contraction or expansion were observed in them, they were interpreted by Müller and Gegenbaur as of the nature of chromatophores. These curious bodies would well repay a renewed examination. According to Panceri (9) they have nothing to do with the phosphorescence exhibited by *Phyllirhoë*.

In Pteropods chromatophores were first observed by Kölliker, Müller, and Gegenbaur (10), and were fully described and figured by the latter naturalist in 1855 (11). Two kinds of chromatophores were recognised. The first type consists of a central pigment-cell surrounded by a close-set series of radial muscle-fibres, each of which possesses a nucleus and has thus the value of a cell. The whole structure is contained within a well-defined space bounded by parenchymatous connective tissue, which forms a wall for the attachment of the radial fibres. The central pigment-cell is expanded by the contraction of the radial fibres as in Cephalopods. This type of chromatophore was found in the fins of several species of *Tiedemannia* and of *Cym-*

bulia quadripunctata. The second type of chromatophore was found in *Tiedemannia chrysotincta*, and consists of a single stellate cell with much branched processes, swollen at their ends. The cell consists of an outer non-contractile membrane and a central hyaline fluid substance containing pigment granules in suspension. The hyaline substance is markedly contractile, and may concentrate itself with the pigment entirely within the central body of the cell, or may expand to the tips of the processes, leaving the central part of the cell empty. The processes frequently contain pigment in one half while the other half is empty.

It is clear that the first of these two types of chromatophore is allied to that found in the Cephalopoda, while the second closely resembles the chromatophore of Vertebrata, as described by Brücke in the Chamæleon, Lister in the Frog, and Ballowitz in the Fish.

With regard to the chromatophores of Cephalopods a mass of controversial literature has accumulated during the last twenty years, and has almost taken the form of another Franco-German war, owing to the way in which the principal investigators have grouped themselves around the central question. Harless's (12) early interpretation of the chromatophore as consisting of a central passive pigment-cell with radial contractile fibre-cells was confirmed by H. Müller, Boll (*l. c.*), and other writers, and has been generally accepted as correct. But Harting (13) in 1874 opposed this interpretation, denied the contractile power of the radial fibres, and concluded that the power of altering the form of the chromatophore must reside within the protoplasm of the central cell itself. He regarded the phase of contraction as the active phase of the chromatophore.

Harting's views have obtained no acceptance except in France, where with slight modifications they have been restated with considerable emphasis, though it cannot be said with satisfactory evidence, by Blanchard (14), Girod (15), and Joubin (16).

On the other hand, the investigations of Kliemensiewicz (17) have furnished a very convincing series of observations

in support of the generally accepted view. It is difficult, in the short space at my disposal, to do justice to this author's valuable memoir, but the following observations made by him may be quoted. The radial fibres are thin and band-like during the contracted condition of the chromatophore, but broader during the expanded condition. In embryonic chromatophores the number of radial fibres is less than in fully developed chromatophores; and, in correlation with this, the chromatophore in expansion exhibits fewer processes or angles in the embryonic condition than when fully developed. Each process of the pigmented cell corresponds to a radial fibre, and has every appearance of having been caused by the pulling out of the membrane of the central cell by the radial fibre at that point. This fact, together with that of the shortening and widening of the radial fibres during the expansion of the chromatophore, renders inevitable the conclusion that the expansion of the chromatophore is determined by the contraction of the radial fibres. Moreover, in spite of Harting's statement to the contrary, there can be no doubt about the fact that the process of expansion of the chromatophore is much more rapid than the process of contraction.

I may add that from observations made at Plymouth during the present summer, I have been able to satisfy myself of the correctness of all these observations, and at the same time to make others which have convinced me that the radial fibres are the active agents in the expansion of the chromatophore. If a piece of the mantle of a young *Loligo media* be placed in a weak solution of methylene blue, the tissues are gradually impregnated with the dye, and the radial fibres are eventually paralysed in the contracted condition. But it frequently happens that the fibres of one side of a chromatophore take up the pigment and are paralysed before those of the opposite side, in which case it is possible to observe a curious phenomenon. One half of the chromatophore remains permanently expanded to its full extent, while the other half displays the usual phases of alternate expansion and contraction. If it be true, as Harting, Blanchard and their supporters maintain,

that the expansion of the central pigmented cell is due to some inherent amœboid power residing in the cell itself, such a phenomenon as this would be in the highest degree remarkable. For it would imply that one half of the cell was dead, or had at any rate lost its normal contractility, while the other half was perfectly healthy. It is more fully consistent with the phenomena to hold that the one half of the chromatophore remains expanded because the radial fibres which are attached to it are contracted and dead, as their deep impregnation with the methylene blue enables one to see, while the other half of the chromatophore is still capable of the ordinary changes of form because the unstained fibres attached to it are still alive and capable of relaxation as well as of contraction. I have even seen instances of chromatophores in which the entire protoplasm of the pigmented cell was dead, and in which the pigment granules were aggregated into irregular patches and streaks instead of being evenly distributed. In such cases it sometimes happened that one or two of the radial fibres, which had taken up relatively little of the stain, could be seen to contract from time to time, entailing a corresponding extension of the central cell in the region of their attachment. The agency of the radial fibres in the expansion of the chromatophore may accordingly be regarded as satisfactorily established.

The exact innervation of the chromatophore has not yet been conclusively demonstrated. Harting's suggestion than the radial fibres are nervous in nature has been seen to be groundless by every subsequent observer; but Kliemensiewicz, in spite of many preparations, was unable to find any nerve-fibrils supplying either the central pigment-cell or the radial muscle-cells. That some kind of innervation exists, however, is perfectly certain from the physiological experiments of Léon Fredericq (18), Kliemensiewicz, and Krukenberg (19). Fredericq showed that the motor centre for the chromatophores resides in the subœsophageal nerve-mass, a conclusion confirmed with an abundance of new detail by Kliemensiewicz in the following year. From the chromatophoric centre pass motor fibres

to the pedal ganglia and their nerves, and to the visceral ganglia and their nerves, while afferent fibres reach the centre from the optic ganglia. Krukenberg's experiments with various alkaloids and muscle poisons showed the probable existence of peripheral ganglia in the skin, as well as of a central organ in the brain. Joubin is the first and as yet the only investigator who has offered a description of the nerves themselves, his results being based on the employment of methylene blue. The chromatophoric nerves of the mantle arise from the stellate (pallial) ganglia, and each branch terminates in a chromatophore. The terminal nerve-fibre, according to Joubin, is slightly swollen at its extremity and abuts directly upon the edge of the pigment-cell between two of the radial fibre-cells. This is the only point in Joubin's description which is not very satisfactorily demonstrated, and it is to be hoped that a point of so much importance may soon be examined again.

The most interesting part of Joubin's paper, however, is his description of the development of the chromatophore in the embryo of *Argonauta*, in which form the development is apparently less abbreviated than in other types of Cephalopod. Joubin shows that the pigmented cell, though ultimately embedded in mesodermal tissue, is originally one of the constituent cells of the embryonic ectodermal epithelium. At an early stage it becomes slightly larger than its neighbours, and then sinks beneath the surface of the epithelium at the apex of a pit-like invagination of the ectoderm. It then enlarges greatly, detaches itself from the epithelial pit, and becomes surrounded by mesodermal cells which transform themselves into the radial muscle-cells. The ectodermal invagination closes up.

In the Cephalopod, therefore, as in the Ctenophore, the pigment-cells are ectodermal; but in the Cephalopod, as in the Crustacean or the Nemertine, the pigment-cells separate themselves from the layer in which they have originated, and take up a secondary position within the mesodermal tissues.

In existing Cephalopods the pigment-cell is a true chromatophore, whose only demonstrable function is that

of contributing to the coloration of the animal. In its final stage it is rendered so complex by the number of accessory nervous and muscular structures which become connected with it, that the question may well be asked by what phylogenetic steps so elaborate an organ has been evolved. As an answer to this question I would invite comparison between the developing chromatophore of *Argonauta* as described by Joubin, and the unicellular "purple-glands" of the mantle-edge of *Aplysia* as described by Blochmann (20). In *Aplysia limacina* each of the purple gland-cells is at first a part of the ectodermal epithelium; it enlarges and sinks beneath the epithelium, retaining only a narrow, neck-like prolongation to the surface; the whole of the cell then sinks deeper within the mesoderm, but its connection with the exterior is retained by the simultaneous development of a multicellular duct of ectoderm cells formed by invagination of the epithelium. Each gland-cell also becomes surrounded by connective tissue cells and muscle-cells, by the contraction of which the pigmented secretion of the gland is forced to the exterior. The striking histological resemblances between these unicellular glands of *Aplysia* and the chromatophores of Cephalopods did not escape Blochmann's notice; but now that Joubin has furnished us with embryological proof of the ectodermal origin of the chromatophore, and has figured an ectodermal invagination which corresponds in all respects to the multicellular duct of the gland-cell in *Aplysia*, it becomes possible to suggest distinct homologies between the two sets of structures. The developmental stages of the chromatophore in *Argonauta* appear to me to be largely recapitulative of its past history; and I would suggest that the chromatophores of Cephalopods have been evolved by the progressive modification of some such complex ectodermal gland as that of the mantle-edge of *Aplysia*. The only serious difference between the two structures is that in the Cephalopod the peripheral muscle-cells expand the pigment-cell, whereas in *Aplysia* they contract it. Kliemensiewicz, however, maintains that in addition to the radial muscle-fibres there is a distinct cellular envelope

around the pigment-cell which is the active agent in the contraction of the chromatophore. These cells, which correspond to the basilar cells of Girod (15), may be the homologues of the peripheral cells in *Aplysia*, while the radially elongated cells may be of more recent origin.

Whatever may be thought of this suggestion, it certainly seems to me significant that complex chromatophores of the Cephalopod type are also met with in certain Gymnosomatous Pteropods, as has been described above,—a group of animals which Pelseneer has conclusively shown to be closely related to the Aplysioid Opisthobranchs.

The Chromatophores of Vertebrata. The chromatophores of fishes have recently been studied from the physiological point of view by Cunningham and MacMunn (21), and from the histological by Ballowitz (22, 23). It is of course with the morphology of chromatophores that we are especially concerned in the present article ; but some of the observations made by Cunningham and MacMunn bear upon one of the points which have been raised. In the pipe-fish *Siphonostoma*, the chromatophores are stated to lie so deep as to form a black sheet over the dorsal surface of the peritoneum ; while in the flounder, *Pleuronectes flesus*, and to a less extent in the dab, *P. limanda*, pigment exists in the form of irregular branched masses between the epidermal cells, often resembling the dermal chromatophores in form, "as though some of the latter had bodily migrated into the ectoderm".

In the eel Waymouth Reid (24) has recognised the cellular nature of the branched pigment masses in the ectoderm, and is strongly disposed to regard them as intrusive mesodermal cells. This is the view which Kölliker (25) was the first to propound as a result of his observations on *Lepidosiren*. In this form numerous pigmented ramifications are found in the epidermis, but the bodies of the cells from which they arise are situated in the superficial layer of the cutis. In the sturgeon, H. Müller found entire pigment-cells within the epidermis, and regarded them as branched ectodermal cells ; but Kölliker suggested that they had intruded from the cutis, *Lepidosiren* presenting a

condition of things which may be interpreted as an early stage in the process of migration.

It is impossible to come to a decision upon this important matter, for embryological observations upon the point seem to be entirely wanting.

Ballowitz's investigations deal partly with the phenomena of movement exhibited by the chromatophores of fishes (22), and partly with the details of their innervation (23). He confirms Solger (26) in his observations that during the contraction of a chromatophore only the inner pigmented protoplasm of the processes is withdrawn, while the outer protoplasmic envelope persists *in situ* in the form of a delicate unpigmented strand radiating out from the central pigmented mass. By the use of Golgi's method these unpigmented envelopes of the radiating processes may be stained in all their ramifications, and can thus be shown to remain permanently fixed, even during the maximum concentration of the pigment within the cell.

Reference may here be made to Guitel's memoir upon *Lepadogaster* (27), in which this investigator has also described the predetermined paths which the pigment takes during expansion of the chromatophore. On Plate xxviii. he figures a chromatophore in different stages of expansion.

Ballowitz further describes for the first time the remarkable nature of the innervation of the chromatophores. The motor nerve-fibres arise from the nerve-plexus in the skin, or frequently from the cutaneous nerves themselves prior to the formation of a plexus. The same motor nerve often supplies many adjacent chromatophores, and the chromatophores are supplied with a very variable number of motor branches, often one, but usually several or many. Peripheral ganglion cells are absent. Near the periphery of the chromatophore the motor fibre usually divides. One branch generally goes more or less directly towards the centre of the chromatophore, the other takes a more peripheral and spiral course over its surface. Both branches divide dichotomously into numerous twigs which terminate in "end-swellings" in the fixed unpigmented envelope of the chromatophore. Moreover, here and there a fibre pene-

trates the substance of the cell and reappears on its lower surface, winding and branching on this surface in a spiral manner in the same way as on the upper.

The chromatophore may thus be described as a disk lying between a pair of horizontal nerve-plates, consisting of spirally wound fibres and terminal swellings, the two plates being here and there in communication through the substance of the cell.

In Amphibia the development of the chromatophores appears to have been almost as little studied as in the case of fishes. The deep pigmentation of the ectoderm in embryos of the common frog supplies some ground, however, for believing that the chromatophores of Amphibia may ultimately be referred to the same embryonic layer, a view which has been defended by Jarisch (28), who maintains that the chromatophores in the epidermis are modified ectodermal cells. Ehrmann (31) states that the epithelial pigments of the Salamander are deposited by wandering cells.

The histology of Amphibian chromatophores has recently been studied by Biedermann (29), whose observations are in close agreement with those of Ballowitz upon fishes, and for the most part confirm the earlier statements of Sir Joseph Lister (30). Biedermann compares the movement phenomena of chromatophores with those of Rhizopods, which possess an external sluggish "hyaloplasm" and an internal more fluid "granular plasm". We may, however, refer to Lister's paper for evidence that the streaming movements of the pigment in Amphibian chromatophores are confined to the pigment granules; the fluid plasm in which they are suspended appears to be left behind in the branches of the chromatophore when concentration of the pigment sets in. Lister adds that the hypothesis which would seem most consistent with the phenomena of expansion is that of a mutual repulsion on the part of the pigment granules induced by some agency strongest at the centre of the cell and feeble in the remotest branches of the offsets. Reading this in the light of Ballowitz's recent demonstration of the situation of the chromatophore between two nerve-

plates, one is tempted to hazard the suggestion that the nervous discharge may actually pass through the chromatophore from one nerve-plate to the other, and after the manner of an electric discharge supply the agency necessary to induce mutual repulsion or attraction on the part of the pigment granules.

In reptiles, Brücke's paper on the Chamaeleon (32) remains classical. It was in this paper that Brücke showed for the first time that during the so-called "contraction" of the chromatophores in Vertebrata, the processes or branches are not themselves withdrawn, but merely the pigment within them.

I have already drawn attention to Kerbert's observations (33) on the development of the chromatophores in snake embryos; the branched pigmented cells appear in the epidermis before they are recognisable in the mesoderm. But Kerbert himself by no means holds that the chromatophores are therefore of ectodermal origin. At the stage when pigment first arises in the ectoderm he finds that both ectoderm and mesoderm contain a number of round or oval cells enclosing a central fluid vacuole; and he interprets these cells as the young stages of the chromatophores. He regards them as connective tissue cells which penetrate into the epidermis, develop pigment granules, and acquire a branched form. The only serious objection that can be urged against this view is that the author does not establish it. He shows us the vacuolated cells in both layers, and the branched pigmented cells in the epidermis; but he does not show us the vacuolated cells becoming chromatophores, although he makes the statement that here and there the vacuolated cells may be seen to be obviously filled with pigment. This is, however, not quite consistent with one of his earlier statements to the effect that pigment first appears in the ectoderm in the form of branched cells only slightly pigmented, the granules being confined to the edges of the cells, and occurring to a slight extent in the processes.

Kerbert finds it difficult to account for the appearance of chromatophores in the ectoderm, since in the adult snake

they are absent from that layer. He dismisses it as probably a "phenomenon of heredity".

The same investigator finds branched pigment-cells in the epidermis of an embryo chick of fifteen days. Here also he regards them as intrusive connective tissue cells, although a difference from those of Viper embryos is found in the fact that the pigment-cells already form pigment while still within the connective tissue. Remarkably enough, the pigment-cells both of the epidermis and of the cutis are stated to disappear completely by the twenty-third day.

In Mammals branched pigment-cells are frequent both in the epidermis and the cutis, but their origin is a subject of much dispute. An excellent summary of the facts may be found in Kölliker (35), who, although inclining to the view that pigment-cells are for the most part of mesodermal origin, does not go to such extremes as Aeby (36), Halpern (37), and others, who deny the pigment-forming power of the ectoderm altogether.

The writings of certain investigators, *e.g.*, Karg (38), Halpern (37), betray signs of a certain physiological preconception as to the function of pigment-cells, which it may be well to deal with here. Karg is especially emphatic upon the great *rôle* which intrusive pigment-cells must play in the nourishment of the epidermis, while Halpern assures us that the cells of the *rete Halpighii* take up pigment through the breaking down of pigment-laden wandering cells, and through absorption of parts of their protoplasm. It is clearly imagined here that the pigments in these cases are supplies of food material; but it may be asked whether the chromatophores of Fishes, Amphibians, and Reptiles are also stores of reserve material. They are well known to be nothing of the kind, and unless we deny the homology between the pigment-cells of Mammals and the chromatophores of the lower Vertebrata, we can afford to be sceptical about views as to the nutritive value of pigment-cells in Mammals. It seems to me to be much more probable that the pigment-cells of Mammals are simply degenerate representatives of the chromato-

phores of the lower Vertebrata. The ancestors of the Mammalia no doubt agreed with existing Amphibia and Reptiles in possessing chromatophores, but these would become useless as soon as the development of hair concealed the skin from view. The same argument may be employed in the case of birds, whose coating of feathers would also render chromatophores of no further use. Hence their degeneracy in birds and Mammals. That they are not necessary for the nutrition of the epidermis is well seen in the case of Primates. In Anthropoid Apes the epidermis contains not only processes from cutaneous pigment-cells, but frequently entire cells as well (Kölliker, 34). In the negro there are no entire pigment-cells in the epidermis, only processes from sub-epidermal cells (Karg, 38; Kölliker, 35). Lastly, in the white races of man pigment-cells are almost entirely absent, and are limited to certain special parts of the body (nipple, scrotum, etc.).

If I have thus rendered it probable that pigment-cells in Mammals have *not* the value of nutritive cells, perhaps this fact may reduce the bias which at present exists towards the view of their imagined mesodermal origin.

It is, of course, perfectly possible that the pigment-cells of Mammals are not homologous with the chromatophores of the lower Vertebrata, and have a nutritive function. That would be a very interesting point to decide.

Here my article must close. The literature upon pigment-cells is enormous, and the study which, for purposes of my own, I have made of it will not only, I trust, lighten the labours of some of my fellow-naturalists who are interested in the subject of animal coloration, but may possibly be serviceable in showing the paths along which new research can most profitably be conducted.

I have been astonished to discover that, so far as I am aware, there is not within the whole of existing literature a single indubitable proof of the mesodermal origin of true chromatic cells, and I have been unexpectedly led by my survey of the facts to the opinion that chromatophores, as defined at the beginning of this article, are universally of ectodermal origin.

How near to the truth, or how far from it, this generalisation is, it must remain with future researches to decide; but it should be borne in mind that the origin of the chromatic cell is by no means necessarily bound up with the origin of the pigment which it contains.

The subjoined classification indicates the relations of the different kinds of chromatophore which have been discussed above.

I. *Autoplastic.* Form-changes intrinsic.
   1. *Holoplastic.* Contractile as a whole, *e.g.*, Ctenophora, Isopoda.
   2. *Endoplastic.* Inner protoplasm alone contractile, *e.g.*, Vertebrata, certain Pteropoda.

II. *Alloplastic.* Form-changes extrinsic, *e.g.*, Cephalopoda, certain Pteropoda.

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WALTER GARSTANG.

THE SPACE RELATIONS OF ATOMS.

(*Continued.*)

THE attempt to advance beyond the distinction between two- and three-dimensional molecules, and to assign a definite form even to the least complex of atomic aggregates has been found full of difficulty. In its simplest form the problem is to find the relative position of the centres of gravity of the atoms in a state of rest, *i.e.*, at the absolute zero. The fact that in the case of a diatomic molecule these points must be in a line, and, in a triatomic molecule, in a plane, does not help us unless we can determine the relative lengths of the lines joining different atom-centres, which at present we cannot. Above the temperature of absolute zero complications are introduced by the motions of the atoms, and as to the extent and direction of these motions we know practically nothing. And in Van't Hoff's view all inferences drawn from the movements of an atom within a molecule are liable to an objection at the outset. The objection is that the moment an atom begins to move within a molecule it is liable to move out of the molecule altogether.

Van't Hoff argues—and the generalisation is as striking as it is simple—that it is only at absolute zero that a molecule is internally stable, seeing that all molecules must be supposed to undergo dissociation at a sufficiently high temperature, and experiment and theory agree in showing that though dissociation diminishes with the temperature, it would never cease short of the absolute zero. Above this temperature, then, the equilibrium of a molecule always depends on its exchanges with others.

The inference is that atomic motion should, for the present, be ignored in our attempts to determine atomic positions. Indeed the argument apparently points to atomic chaos as the only state possible above absolute zero ; but the inter-molecular exchanges may be supposed

to take place always at the same phase of the oscillation of an atom so as not to interfere with the periodic nature of the oscillations, and Van't Hoff admits that "he who prefers to assume atomic motion may consider the points, described as stationary, as positions about which the motion, probably periodical, takes place".

This has from the first been the attitude of Van't Hoff, though the doctrine of universal dissociation appears only in the latest edition of his book (1894); and it was natural that in its inception stereochemistry should take this trend, seeing that its first task was to account for cases of "abnormal" isomerism, and that it was only later it began to concern itself with "abnormal" reactions, and therefore with the problem of atomic dynamics.

It was the discovery by Wislicenus in 1869 of a second active lactic acid, which, following on the work of Pasteur already mentioned, forced chemists to see the insufficiency of the ordinary structural formulæ, of which there is only one for these two substances, *viz.*, $CH_3.CH.OH.COOH$.

At this time attempts at three-dimensional formulæ had already been made; various chemists had devised models for the purpose of illustrating the binding-power or valence of the various atoms, each binding-unit being represented by a rod attached to the sphere which stood for the atom; these rods were generally all in one plane, and Kekulé was the first (1867) who, in order to represent the power of a carbon atom to combine with another by one, two, or three of its units of valence, directed the rods in this case towards the corners of the tetrahedron described about the carbon-sphere. For a time the influence of Wislicenus' discovery on such speculations was not apparent; thus in 1873 there was published Gaudin's *L'architecture du monde des atomes dévoilant la structure des composés chimiques et leur crystallogénie*,[1] in which the author is guided entirely by certain laws of symmetry similar to those which apply in crystallography; and in 1875 F. W. Clarke[2] in a paper called "Chemistry in Three Dimensions" argued that the monatomic

[1] Paris, Gauthier Villers.

[2] *Proc. Am. Ass. Adv. Sci.*, p. 99, 1875.

atoms must always occupy the periphery of the molecule because of their readiness to react, and from the forms of crystals he concluded that all molecules must possess three dimensions. These speculations only served to illustrate the sterility of theories which cannot be submitted to definite practical tests.

Meanwhile, in 1873, Wislicenus had published in greater detail the proofs that the two active lactic acids possess the same structural formula, and had again urged, as he had done earlier, that the difference in their properties must be accounted for by a difference in the space relations of their atoms. At this point, and inspired by this doctrine, Van't Hoff took up the subject in 1874; a little later in the same year Le Bel's first paper appeared; and the results of Pasteur, of Kekulé, and of Wislicenus appeared as connected parts of one harmonious whole, when seen in the light of the theories thus almost simultaneously published, and now recognised as the basis of modern stereochemistry.

The basis of Van't Hoff's theory was a tetrahedral configuration for the groups attached to a carbon atom; this shows two configurations for the active lactic acids (fig. 5,

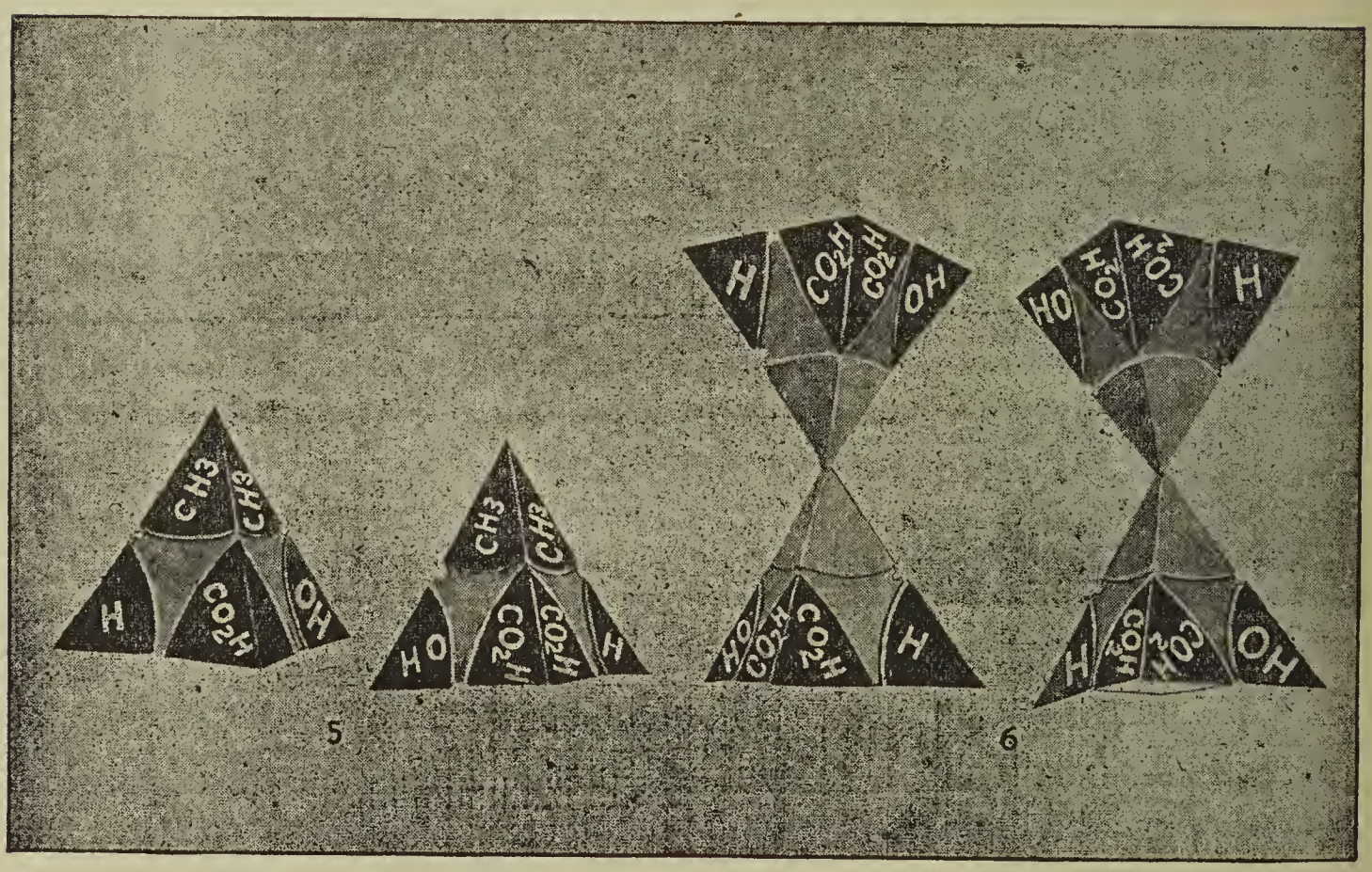
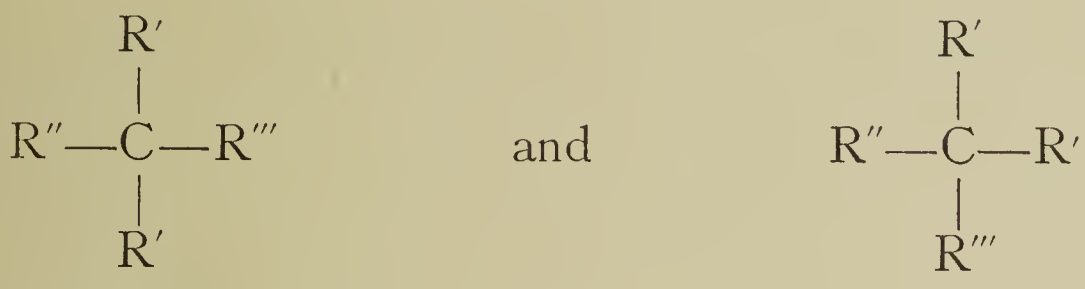


PLATE I.

Plate I.), and in general two configurations for $CR'R''R'''R^{iv}$ which become identical when two R-groups become iden-

tical, giving only one $C(R')_2R''R'''$; whereas a plane formula must have at least two configurations for this substance, representing two isomers :—



where only one actually exists. For a similar reason preference must be given to the tetrahedral grouping for $C(R')_2(R'')_2$; for $C(R')_3R''$ and for $C(R')_4$ both the solid and the plane formulæ accord with the facts in representing only one configuration ; but for these, too, Van't Hoff prefers the former.

"My fundamental idea was the tetrahedral grouping, that is to say, some force—cause unknown—proceeding from the carbon atom and tending to drive the groups united to carbon as far away from one another as possible, that is, to bring them into the tetrahedral position. Although it did not follow that the tetrahedron must be regular, because the mutual action of the different groups may vary it somewhat, yet the tendency to form the regular tetrahedron remained, and in the case of identity among the groups, as in CH_4, the tendency was realised" (*Atome im Raume*, p. 68).

As to this, Le Bel holds that it depends entirely on the relation between the attraction of C for H on the one hand, and the repulsion between H and H on the other. He assumes [1] that atoms attract one another up to a certain point, but on closer approximation repel one another. We have then for CR_4 two positions of equilibrium of R_4 about C. A symmetrical (regular tetrahedral) arrangement will result if the repulsive zones of R occupy the whole surface of the zone C or a greater surface ; but if less, a non-symmetrical arrangement will result. (They

[1] *Bull. Soc. Chim.* [3], iii., 788.

cannot occupy much less surface, because then another R would have room to attach itself to C.) The first arrangement has the symmetry of the cubic system, the second that of the rhombic system; and for CR_3R' we should have a rhombohedral arrangement. And in fact we find CI_4 to be tetrahedral, CBr_4 rhombic, and $CHBr_3$ rhombohedral. But in the present state of our knowledge of the relation between chemical constitution and crystalline form, little weight can be attached to such coincidences.[1]

That the carbon atom really possesses a repulsive force, Le Bel infers from the ratio of the specific heats of marsh gas, which shows that the atoms do not touch; and he points out that, for carbon at least, the hypothesis of the repulsive zone dispenses entirely with the necessity for a valence-hypothesis. His objection to the valence-hypothesis is that, as ordinarily held, it attributes to the carbon atom, not merely an attractive force, but a directive force acting at right angles to this, and causing the attracted groups to take the tetrahedral position.

It will be seen from this paper, written by Le Bel to show how his views differ from those of Van't Hoff, that this difference is not so great after all. Le Bel's "repulsive zone" is represented by Van't Hoff's "force proceeding from the carbon atom, and tending to bring the groups connected with it into positions as far removed from one another as possible, that is" (but here Le Bel would add the qualification "unless the ratio of the repulsive zones is unfavourable") "into the tetrahedral position".

And, to quote another passage of Van't Hoff: "Historically the difference lies in this, that Le Bel's starting-point was the researches of Pasteur, mine those of Kekulé. . . . My conception is a continuation of Kekulé's law of the quadrivalence of carbon, with the added hypothesis

[1] For an account of the work of Sollas, Muthmann, Tutton, and Penfield on the arrangement of atoms in crystals, the reader is referred to the article by H. A. Miers in "SCIENCE PROGRESS" (iii., 129).

that the four valences are directed towards the corners of a tetrahedron, with the carbon atom in the centre. . . ."[1]

"Practically our ideas, so far as they concern the asymmetric carbon, amount to the same thing; explanation of the two isomers by means of the tetrahedron and its image, disappearance of this isomerism when two groups become identical through the resulting symmetry and identity of the two tetrahedra" (*Atome im Raume*, p. 2, 1894). As to whether an asymmetric atom—to the exclusion of an asymmetric group—is necessary for optical activity, Van't Hoff from the first regarded this question as settled in the affirmative by the test of experiment; as to why an asymmetric group should not be equally effective he offers no theory. With regard to the question whether an asymmetric atom is sufficient to cause optical activity Van't Hoff will admit of no exceptions; the apparent exceptions are due in his view entirely to the difficulty of "doubling" the inactive mixture.

The right- and left-handed relation of the isomers $CR'R''R'''R^{iv}$ in their action on light and their crystalline form has already been discussed.

It is to be noted, further, that all the molecular dimensions being equal in the two isomers, we must expect a kind of isomerism distinguished by a near approach to identity. Accordingly we find that both isomers possess the same specific gravity, critical temperature, boiling-point, melting-point, latent heat of fusion and vaporisation; in short, all the physical properties depending on molecular dimensions and attractions are the same.

As regards chemical properties, we find equal stability, the same speed of formation, equilibrium when equal quantities of each are present together, and equal heat of formation. This quantitative proof raises to mathe-

[1] Kekulé had already suggested this arrangement of the carbon-valences, as we have seen; but the great advance made by Van't Hoff was that he showed the practical bearing of this hypothesis on optical isomerism, on geometrical isomerism, and on ring-formation, in every case bringing the hypothesis to the test of experiment.

matical certainty the evidence in favour of the enantiomorphic formulæ assigned to the two isomers.

Various models have been devised to illustrate the tetrahedral configurations, and it cannot be too strongly urged that models of some kind are essential to a clear understanding of the subject. Van't Hoff used cardboard tetrahedra, with corner-caps differently coloured to represent the different groups. By placing four caps of different colours, in different order on each of two tetrahedra, we obtain the two asymmetric configurations related as object and image, representing the two isomers $CR'R''R'''R^{iv}$. But if we make two of the corner-caps of the same colour on each tetrahedron the two configurations become identical, and each possesses now a plane of symmetry. The same thing may be represented by the Kekulé models as modified by v. Baeyer.

A simple and inexpensive form of model has been devised by P. Friedländer,[1] and consists of four short pieces of caoutchouc tubing, symmetrically diverging from a centre where they are joined together (according to Van't Hoff with sealing-wax).

I am indebted to Mr. H. A. D. Jowett for a description of similar models, which any one may readily make for himself by attaching short rubber tubes plugged with cork to four equidistant points on a small, hollow rubber ball by means of tacks (head inside the ball) and rubber cement. This arrangement is of advantage if one wishes to represent the carbon atom as having an appreciable size compared with the distance between the atoms. Moreover, these models are more easily made of the requisite symmetry than are Friedländer's.

Another method is to solder together at the ends two brass rods, each bent at the middle at an angle of 109°; coloured rods, or balls, or flags bearing formulæ may be attached to the ends of the brass rods by means of rubber connectors.

While for private study it is desirable to use models as small as can be conveniently handled, for lecture purposes

[1] *Ber. d. Chem. Ges.*, xxiii., 572.

we need them as large as can be conveniently handled, and we may then use tetrahedra having corner-caps marked with the formulæ of the particular radicals attached to the carbon atom. Models of this kind have been described [1] under the name of "solid formulæ," and some of the plates illustrating them are reproduced here to represent the configuration of certain molecules.

To illustrate the fact that unlike groups are at unequal distances from the central carbon atom, models with unequal sides or limbs would be required, but for ordinary purposes the symmetrical models suffice, and are in fact always used.

With the aid of the models we may now follow Van't Hoff in his development of the formulæ of molecules containing connected carbon atoms.

Assuming that the radicals attached to an asymmetric carbon atom have the tetrahedral configuration, we must represent a compound with two connected carbon atoms, $CR'R''R'''$ $CR^{iv}R^{v}R^{vi}$, by two tetrahedra, each carbon atom occupying at once the centre of one tetrahedron and the corner of the other. In the models the tetrahedra are, for convenience, shown simply corner to corner (see the solid formulæ for tartaric acid, figs. 6 and 7, Plates I. and II.). Any other configuration obtained by the rotation of either tetrahedron about the axis C — C would be equally in accord with the fundamental conception; but we shall not therefore encounter innumerable isomers, for there will be only one of these configurations realised at any given temperature, *viz.*, the one favoured by the mutual action of the groups $R'R''R'''$ and $R^{iv}R^{v}R^{vi}$ at that temperature ("favoured configuration"). As change of temperature affects different groups differently, it is evident that if the change be carried far enough another configuration may become the favoured one, and there is evidence that in some cases this happens.

We may for convenience assume the configuration favoured at ordinary temperatures to correspond to the formula,

[1] Eiloart: *Am. Chem. Journ.*, xiii., 559; also *A Guide to Stereochemistry*. London: Clarence Eiloart, 1893. These models are now made by Messrs. Baird & Tatlock, according to samples approved by the writer. Models of the Friedländer type are supplied by the same makers.

$$\begin{array}{c} R''' \\ R'CR'' \\ | \\ R^{iv}CR^{v} \\ R^{vi} \end{array}$$

in which R′ is over R^{iv}, R″ over R^{v} and R‴ over R^{vi}. This plane formula may be considered as a projection of the model in which the front groups R‴ and R^{vi} are turned upwards and downwards respectively. "These projections may be used for illustrating the possible isomers. That there will be four of these will be at once evident, since each asymmetric carbon atom involves a doubling. These differences are represented by changing the order of the groups R′R″R‴. But if, without changing the order, we simply move R′ to R″, R″ to R‴, R‴ to R^{iv}, we only bring about the above-mentioned rotation, and no isomerism results. But if R′ and R″ change places we get a new isomer, also by transposing R^{iv} and R^{v}; hence the following symbols represent the four isomers. It is plain that these are reduced to two directly the asymmetry of one of the carbon atoms ceases, through R^{v} and R^{iv} becoming identical."

No. 1.	No. 2.	No. 3.	No. 4.
R‴	R‴	R‴	R‴
R′CR″	R″CR′	R′CR″	R″CR′
$R^{iv}CR^{v}$	$R^{iv}CR^{v}$	$R^{v}CR^{iv}$	$R^{v}CR^{iv}$
R^{vi}	R^{vi}	R^{vi}	R^{vi}

(Van't Hoff, *l. c.*, p. 37).

With three carbon atoms another doubling results, and there should be eight isomers, of which the formulæ may readily be constructed. And in general, for *n* carbons there should be 2*n* isomers.

As to the character of the isomers, the formulæ show that they must occur in pairs, thus No. 1 and No. 4, No. 2 and No. 3, in the above series are enantiomorphous and alike in chemical properties, and in all physical properties except those which depend on their enantiomorphism; but each of the two pairs will differ from the other in activity, melting-point, solubility, etc.

In confirmation of the theory may be cited borneol and

its derivatives, which contain two carbon atoms, and exist in four isomeric forms occurring in two pairs, thus :—

r- and *l*-borneol *a* rotation $[a]_D$ = + and − 37°.

r- and *l*-borneol β rotation ,, = + and − 33°.

Compounds containing three asymmetric carbon atoms should present eight isomers belonging to four types, and there have been actually prepared three types corresponding to the formula $CO_2H(CHOH)_3CH_2OH$, *viz.*, arabonic acid, xylonic acid, and ribonic acid. The corresponding acids with four asymmetric carbon atoms, $CO_2H(CHOH)_4CH_2OH$, should exist in sixteen different forms belonging to eight types; of these, eight isomers belonging to five types are known.

When the formula containing the asymmetric carbon is symmetrical the number of isomers is reduced, as identity results between what may be called the right-left and the left-right combinations, so that with the left-left and the right-right compounds we have altogether only three isomers for two asymmetric carbons; it has already been seen that this is the number of isomers presented by the symmetrically constituted compounds, tartaric acid and hydrobenzoïn. A corresponding simplification results for more than two asymmetric carbons.

To determine which configuration to assign to each isomer is often difficult, and to decide which of the two enantiomorphic symbols belongs to the right-rotating and which to the left-rotating compound is impossible. But in the case of tartaric acid, *e.g.*, we recognise the symbol

$$\begin{array}{l} CO_2H \\ HCOH \\ HCOH \\ CO_2H \end{array}$$

, as belonging, on account of its symmetry, to the "inactive indivisible" type. This enables us to assign to that type of tetrose, $COH(CHOH)_2CH_2OH$, which yields inactive tartaric acid on oxidation, the symmetrical formula

$$\begin{array}{l} H_2COH \\ HCOH \\ HCOH \\ OCH \end{array}$$

; while the other type, yielding active tartaric acid,

must have the formula $\begin{matrix} H_2COH \\ HCOH \\ HOCH \\ OCH \\ HCO_2H \end{matrix}$, from which only the non-symmetrical acid $\begin{matrix} HCOH \\ HOCH \\ CO_2H \end{matrix}$ can be derived. (Each of these types of tetrose corresponds with two isomers, right and left.) Through the experimental work of Fischer and Kiliani it has even been found possible to assign its proper formula to each of the seven known glucoses, $CH_2OH(CHOH)^4COH$, the guiding-thread being always the connection between inactivity and symmetry of formula among the derivatives, and between activity and lack of symmetry.

The configuration of the unsaturated compounds $CR'R''$ $CR'''R^{iv}$ is represented by placing the tetrahedral models edge to edge (see the solid formulæ for fumaric and maleïc

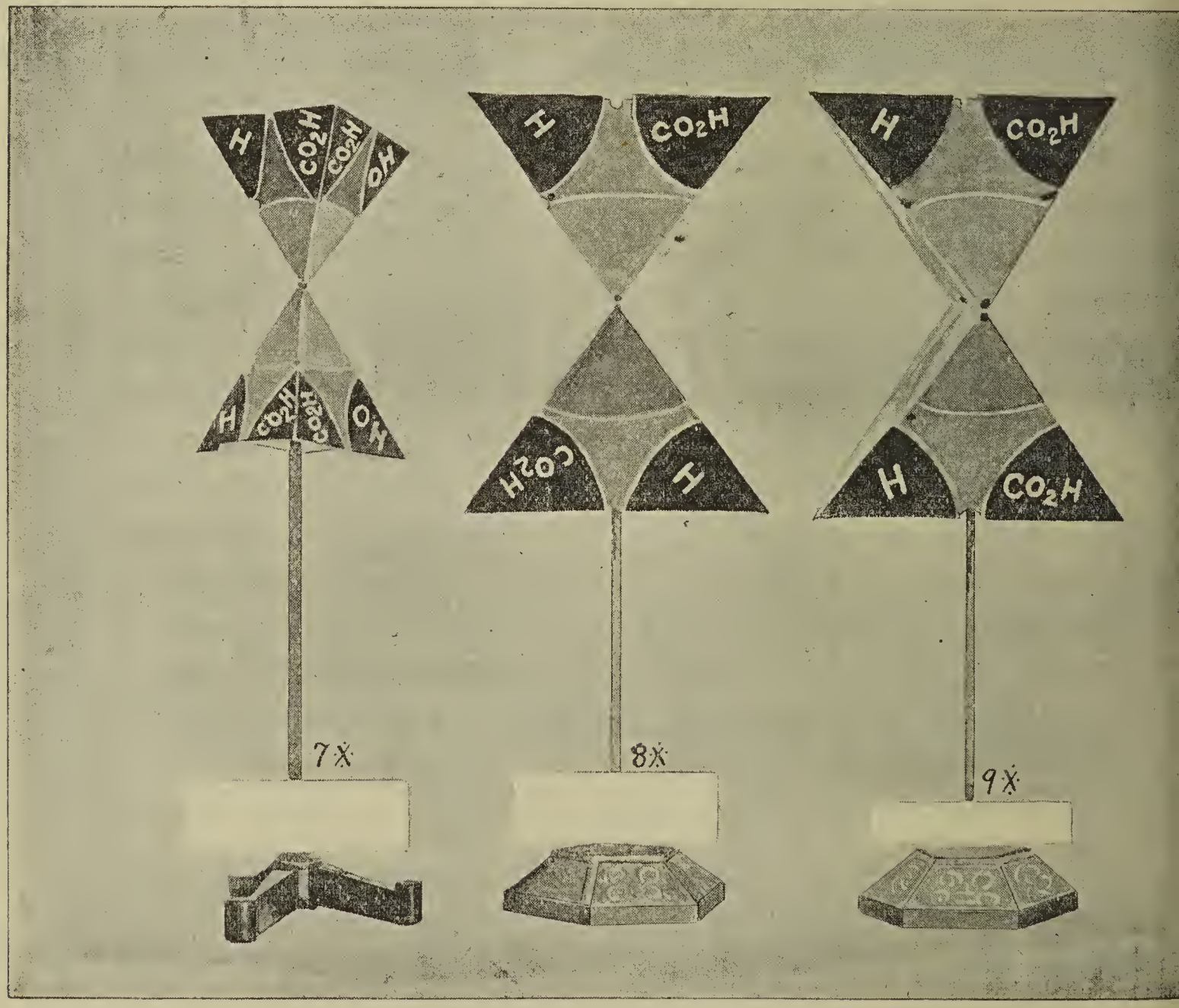


PLATE. II.

acids, figs. 7 and 8, Plate II.), although theoretically the tetrahedra should intersect to show the *rapprochement* of their centres. It will be observed that the R groups occupy one plane with the carbon atoms—a result foreseen by the tetrahedron-hypothesis (1874), long before it was proved (1882) by the reasoning already set forth. It is evident also that the tetrahedral configurations indicate the existence of the "cis-trans" isomers, which, without recourse to the models, may be indicated by the formulæ :—

$$\begin{matrix} R'CR'' \\ \| \\ R'''CR^{iv} \end{matrix} \qquad \text{and} \qquad \begin{matrix} R'CR'' \\ \| \\ R^{iv}CR''' \end{matrix}$$

The same isomerism will exist for $CR'R''$ $CR'R''$.

The character of this isomerism will be entirely different from that due to the presence of an asymmetric carbon. There is neither dissymmetry nor enantiomorphism in structure, and we shall expect therefore neither opposite optical activity nor hemihedry, and in fact we find none.

It has been made a reproach to stereochemistry that the same theory which explains the purely physical isomerism of the substances $CR'R''R'''R^{iv}$ is used to account for the pronounced chemical differences which characterise the isomers $C_2R'R''R'''R^{iv}$. But the new isomers lack the equality of molecular dimensions formerly observed; the theory is therefore perfectly consistent in demanding that they should differ in chemical as well as in physical properties. And Van't Hoff is now able to enumerate thirty-six cases of isomerism characterised by differences of this kind among bodies of the formula $C_2R'R''R'''R^{iv}$.

Van't Hoff was able at the outset to determine in certain cases which of the two formulæ belonged to each isomer, and since then Wislicenus has successfully attacked this problem in other cases. To this end these chemists considered in the first place the mechanism of the addition of atoms to a molecule, and in the second place the mutual influence of the indirectly connected groups.

In considering the addition of atoms to a molecule they relied on the principle of "least disturbance"; the structure of the molecule must be as little altered as possible. We

have already considered the addition of two hydroxyl groups to fumaric and maleïc acids respectively, and the proof that the acid (maleïc) which yields inactive tartaric acid

$$\begin{matrix} \text{OH} \\ \text{H}C\text{COOH} \\ \text{H}C\text{COOH} \\ \text{OH} \end{matrix}$$

must possess the "cis" formula

$$\begin{matrix} \text{H}C\text{COOH} \\ \text{H}C\text{COOH} \end{matrix}$$

depended on the assumption of the above principle, for it assumed that both the OH groups attach themselves to the same side of the molecule; or, in other words, that only one pair of tetrahedron-corners is separated to take up the two added groups; similarly the "trans" formula is proved for fumaric acid (compare the figures for fumaric, maleïc, and tartaric acid).

In considering the effect of adding atoms to two carbons trebly linked, we must remember that these are represented by two tetrahedra placed face to face, and that when one pair of corners is separated to take up two new atoms, two pairs of corners will remain joined, so that the added atoms will be on the same side of the molecule. On addition of bromine to acetylene-dicarboxylic acid we should get, then, $\begin{matrix} CO_2HCBr \\ CO_2HCBr \end{matrix}$, the "cis" or dibrom-maleïc acid. As a fact we get this acid, but at the same time there is formed twice as much of the other isomer $\begin{matrix} CO_2HCBr \\ BrCCO_2H \end{matrix}$; the formation of the latter is, however, not surprising when we consider that it is much more stable than its isomer, which may be supposed to be the sole immediate product, and to undergo transformation while in the nascent state. The numerous similar objections to the theory which have been raised, especially by Michael, are similarly answered, for in every case the objection is the same, that instead of the expected product there is formed another which is more stable in the conditions of the experiment. Moreover, these objections refer exclusively to halogen-derivatives, which we have seen to be especially liable to transformation.

As when atoms are added to the molecule, so when they are abstracted from it, a difference between the isomers is

noted which may also be said to depend on the principle of least disturbance. The molecule $\begin{matrix} R'CR'' \\ R'CR'' \end{matrix}$ will suffer less disturbance in becoming $\begin{matrix} CR'' \\ CR'' \end{matrix}$ than will the molecule $\begin{matrix} R'CR'' \\ R''CR' \end{matrix}$; and accordingly it has been found by Wislicenus and his pupils that the rate of reduction in the same conditions is greater in the former case.

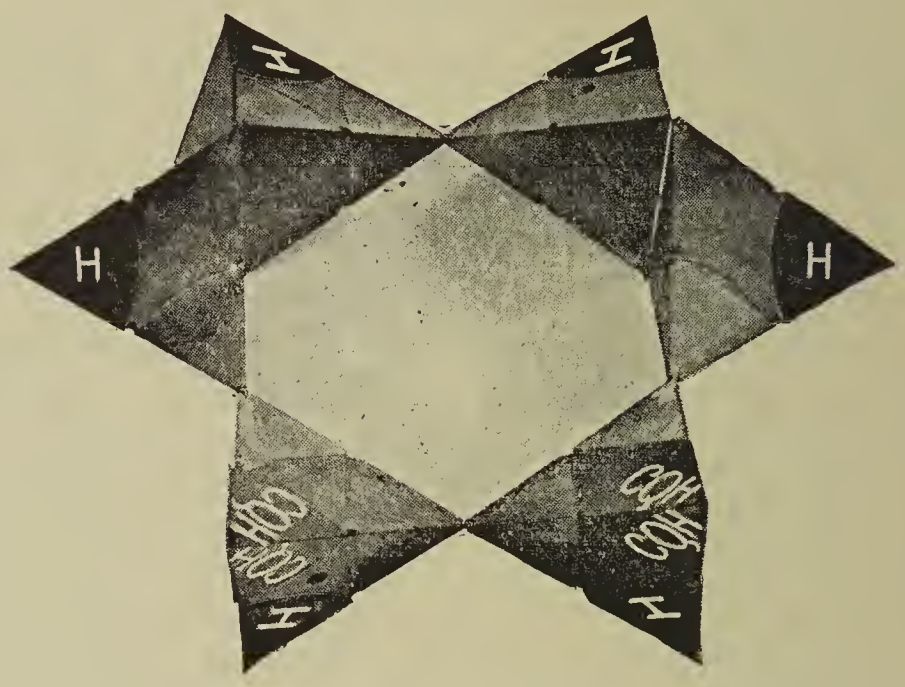
With regard to the mutual action of the R groups, we may say that the formula showing those groups which have the greater attraction for each other as occupying the same side of the molecule, will belong to the more stable isomer. Wislicenus assumed that the degree of attraction varied directly with the difference in positivity of the radicals R. But the matter appears to be not so simple. If, however, we have reason to assign to the less stable isomer the formula $\begin{matrix} R'CR' \\ R^{iv}CR''' \end{matrix}$, and to the more stable $\begin{matrix} R'CR'' \\ R'''CR^{iv} \end{matrix}$, we may test this by determining the heat of formation, which will always be greater for that isomer which is in an absolute sense the more stable; the proportion of this isomer formed at any given temperature may not be the greater, but as the temperature is lowered its proportion will increase until ultimately it preponderates.

Again when two groups in a compound readily react together, we attribute to them the "cis" position; hence, from its tendency to form the anhydride, maleïc acid must have this formula. The same conclusion may be drawn from its formation from ring-compounds; and that the negative groups are closer together in this compound than in its isomer is shown by the fact that its dissociation constant is twelve times higher.

The application of the tetrahedron-hypothesis to ring-compounds is illustrated by the figures of trimethylene derivatives, published by Van't Hoff twenty years ago; here the R groups occupy two planes, one on each side of that of the carbons; thus the arrangement for the trimethylene dicarboxylic acids which Buchner deduced, as we have seen, independently of the tetrahedron-hypothesis,

was long ago foreseen by it. The figures show also how "cis-trans" isomerism occurs in ring-compounds; when R′ changes places with R″ we get the other isomer.[1]

PLATE III.

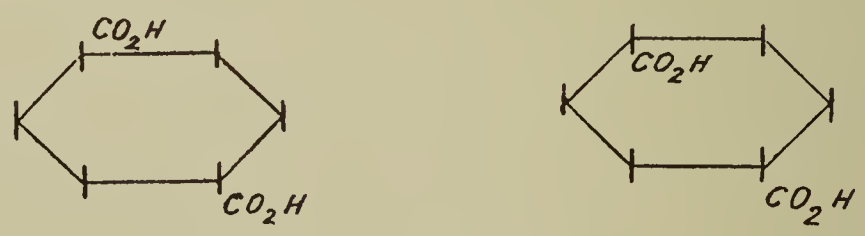


Among tetramethylene derivatives this kind of isomerism is encountered in the truxillic acids

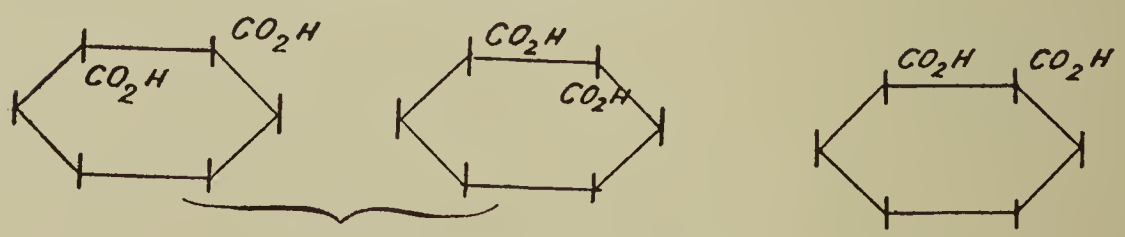
$$\begin{array}{cc} C_6H_5CH-CHCO_2H & C_6H_5CH-CHCO_2H \\ \quad\quad | \quad\quad | & \text{and} \quad\quad | \quad\quad | \\ C_6H_5CH-CHCO_2H & CO_2HCH-CHC_6H_5 \end{array}$$

of which at least four isomers are already known, and for which the theory foresees the existence of many more.

In every case where the R groups become identical, the isomerism vanishes; thus in the monosubstituted polymethylene derivatives it is non-existent. Again, the theory is borne out by the hexamethylene derivatives, hydroterephthalic and hydrophthalic acids, which may be shown in skeleton, thus :—



Hydroterephthalic acid.

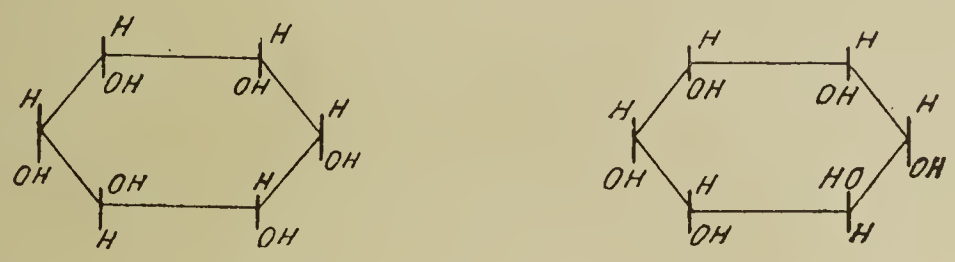


Hydrophthalic acid.

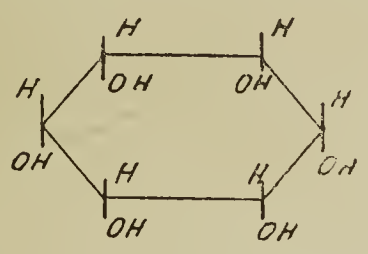
It will be seen that in the last case enantiomorphism exists, together with the "cis-trans" isomerisms, a complication

[1] Similarly in Plate III., hydrophthalic acid, if CO_2H and H be transposed.

observed in many other cases also. And in another hexamethylene derivative, inosite, which also possesses this kind of asymmetry, right- and left-handed isomers have been prepared.

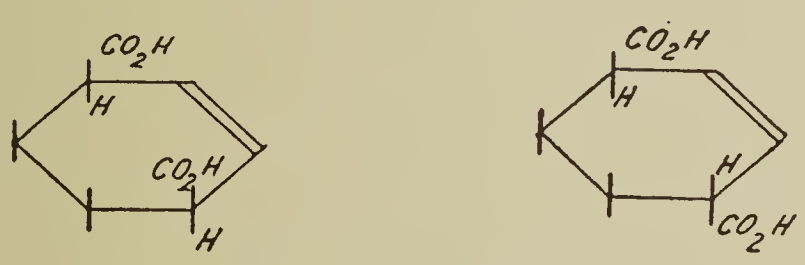


The inosite formerly known being inactive and in divisible must have a symmetrical formula, thus :—



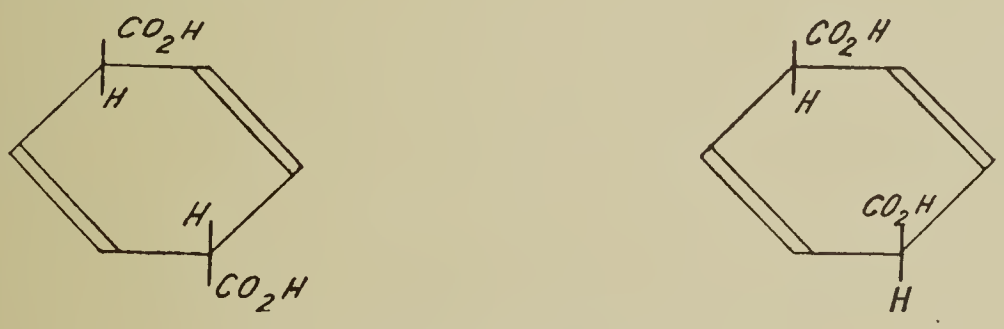
Other active isomers corresponding to other asymmetric formulæ have yet to be discovered.

Passing from hexamethylene derivatives to tetrahydrobenzol we still find stereo-isomers ("stereomers"), corresponding to the formulæ



Tetrahydroterephthalic acid.

And the isomerism continues so long as we have two R′R attached to the ring



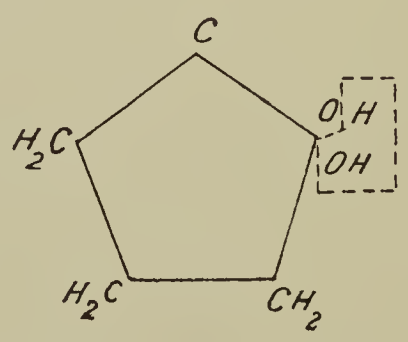
Dihydroterephthalic acid.

But just as the stereomerism of ethylene derivatives disappears when they are reduced to acetylene derivatives, so the stereomerism of the hydrobenzene derivatives disappears when they are reduced to benzene derivatives. And the reason is probably in both cases the same; the atoms are reduced to one plane; again the tetrahedral construction

is in accord with conclusions drawn independently of it from the observed inactivity of benzene derivatives.

The relative stability of ring-compounds, no less than the isomerism they present, has been attributed to the space relations of the carbon valences. If a chain of tetrahedron models be formed by joining them at one corner, it will be seen that, without bending the connecting rods, a free corner of the fifth may be made to all but touch a free corner of the first; so that to close the ring requires but a slight divergence of the linking valences from the angle of the regular tetrahedron, in fact the divergence is only 3°. In forming a hexa-, tetra-, tri- or di-methylene ring the divergence would be 11°, 19°, 49°, and 109° respectively; and it has been observed that the ease with which a ring is broken, corresponds to the divergence in question. Thus penta-, hexa-, and tetra-methylene rings are the most stable, whereas the trimethylene ring may be broken by means of bromine, and dimethylene is saturated even by iodine. Baeyer has applied the same theory to account for the explosive nature of acetylene and especially of polyacetylene compounds.

Returning to the chain of singly-linked carbons, we see that as the groups attached to the fifth carbon are nearer to the first carbon than are the groups attached to the fourth, so these are nearer than the groups of the third carbon. Accordingly we find that in an open chain of four carbons it is the groups attached to the end carbons, those which appear in an ordinary formula farthest apart, which are the readiest to react; *e.g.*, the γ- and δ-oxy-acids, which contain three and four carbon atoms between OH and COOH, are the readiest to form lactones in accordance with the formula



On making a chain of models with alternate double and single linkage, it will be found that the sixth carbon atom almost touches the first, thus indicating the superior stability of the benzene ring.

ARNOLD EILOART.

THE SYNTHESIS OF PROTEIDS.

SINCE Wöhler, in 1828, succeeded in making urea artificially from its elements, the strides that organic chemistry has made have been prodigious. Complex substances previously made only in the living laboratory of plants and animals are now manufactured in the test tubes and retorts of the chemist. The substances which are of most importance to vital processes, the carbohydrates and the proteids, are among the last to yield before this advance. Fischer has, however, shown the way in which sugar may be made, but the synthesis of proteids, the most complex of all the compounds of carbon, is still not accomplished. There are, however, signs that this last conquest of organic chemistry cannot be far off, and when it has taken place we shall be nearer the settlement of many problems that now perplex the physiologist and the economist than we are at present. The vexed question of the constitution of albumin will be set at rest; light will be thrown upon many physiological processes that are at present obscure, and we shall be on the road to determine with accuracy the components of protoplasm, perhaps even in the distant future the manufacture of living material itself will not be such a hopeless task as it appears to be now. Economists who paint terrible pictures of how in a few centuries the land will be unable to support the increased population of the globe, will be comforted if only it is shown them that chemists will be able to make the substances which up to now we have relied upon nature to provide us with.

I propose in the following paper to briefly sketch one or two of the principal attempts that have been made in the manufacture of albuminous from simpler substances.

The products of decomposition of a proteid are extremely numerous, and vary with the method adopted for their decomposition. Briefly they fall into two groups, the fatty compounds generally containing an amidogen radicle, and the aromatic compounds or derivatives of benzene. Our

knowledge concerning these decompositions has been advanced by numerous chemists and physiologists, references to whose works will be found in a paper by Dr. Brodie in the September number of this journal. Among the names there mentioned it will be seen that Schützenberger's figures very largely, and to this observer belongs the credit of an attempt (one of the earliest conducted on scientific lines) to build up from the compounds he had obtained from albumin, something like the original proteid he had broken up (1).

In order to effect the synthesis of proteid material, he considered it necessary to combine a molecule of a leucine (*i.e.*, an amido-fatty acid) with a molecule of a leuceine (an amido-acid of the acrylic series) with elimination of water, and then to combine this complex group with one or more molecules of urea, and oxamide, also with elimination of water. The method he had adopted for the breaking up of proteids was boiling with alkalis; this led to hydration, so in any attempt at synthesis he recognised as a *sine quâ non* the necessity of some method of dehydration.

The provisional formula he gives is the following :—

$H_2C_2O_4 + 2NH_3 + 3C_mH_{2m+1}NO_2 + 3C_nH_{2n-1}NO_2$ with elimination of eight molecules of water. This would give $C_{q+2}H_{2q-8}N_8O_8$, and if $q = 28$ the percentage composition calculated from the formula agrees closely with that of albumin.

Accordingly amido-compounds, leucines ($C_mH_{2m+1}NO_2$) and leuceines ($C_nH_{2n-1}NO_2$), were mixed with about 10 per cent. of urea and finely powdered. The mixture was dried at 110° C., and intimately mixed with 1·5 times its weight of phosphoric anhydride, and heated in an oil bath. At 120° there is no change, but at 125° dehydration takes place very rapidly, and the mixture becomes pasty, but solidifies to a compact product without any darkening. This was dissolved in water, the solution mixed with excess of alcohol, and the pasty precipitate so produced washed with alcohol and redissolved in water. Phosphoric acid was removed by means of baryta, and the filtered liquid when concentrated on a water bath, yielded an amorphous pro-

duct soluble in water, but was precipitated as a curdy mass on the addition of alcohol. Aqueous solutions of this product are precipitable by most of the other precipitants of proteids, namely, tannin, picric acid, mercuric chloride, Millon's reagent, potassium diiodide, mercuric potassium iodide, phosphotungstic acid in presence of hydrochloric acid, phosphomolybdic acid, and lead acetate and basic lead acetate. Potassium ferrocyanide, however, gives no precipitate in presence of acetic acid. With caustic potash and copper sulphate a rose-red coloration is formed. Heated with nitric acid the product becomes orange on adding ammonia, and when heated decomposes suddenly leaving a bulky mass of carbon. Its behaviour in this respect is similar to that of gelatin. When heated on platinum, the compound carbonises and swells up, giving the characteristic odour of burning nitrogenous animal matter.

We thus see that although Schützenberger succeeded in obtaining a substance very like albumin, yet the experiments are hardly conclusive because some of the characteristic properties of albumin are wanting, and the colour tests for proteid are given by many of the decomposition products of albuminous matter. His partial success will, however, point the way for future attempts, and so far as it goes is in favour of his theory of proteid constitution.

Some years previous to this, Grimaux (2) obtained by somewhat simpler processes substances which even more resembled proteids than Schützenberger's. He was especially interested in colloidal substances, inorganic and organic, but the three that he made which bear on the present question were the following :—

(A) Colloïde amidobenzoique. This is made by heating to 125° C. meta-amidobenzoic acid in sealed tubes with one and a half times its weight of phosphorus pentachloride for ninety minutes. The product, which is a white, friable powder, is washed repeatedly with boiling water to remove all phosphoric acid. The remaining substance is supposed by Grimaux to be an intramolecular anhydride formed by the union of several molecules of meta-amidobenzoic acid with the elimination of water. When ammonia is added it

dissolves slowly in the cold, but rapidly on heating. The solution obtained should be evaporated *in vacuo* at a low temperature. The resulting solid is a transparent jelly which dries into translucent, yellowish plates, which in their physical properties resemble dried serum-albumin.

(B) This colloid is similarly prepared, except that the temperature in the sealed tubes is allowed to rise to 135° C.

(C) Colloïde aspartique is prepared by the action of a current of gaseous ammonia heated to 170° C., on solid aspartic anhydride. The product is washed with water, and after evaporation *in vacuo* yields a substance similar in appearance to the colloid (A).

It will be seen from this that the obtaining of albumin by these methods was hardly to be expected, but rather one would get a product which would be, as it were, a skeleton of a proteid; in all cases heavy molecules were formed; in all cases the result was a colloid substance exhibiting, as we shall see directly, many of the properties hitherto deemed diagnostic of proteids, and in the case of the two first colloids there was present not only the amidogen, but also the aromatic radicle.

The resemblance between the proteids and these synthesised colloids is, however, remarkably close, and Dr. J. W. Pickering, who has been instrumental in bringing Grimaux's work prominently before English physiologists, has confirmed most of his results, and also discovered certain other similarities which were not noted by Grimaux. I take the following brief *resumé* of the chief of these similarities from Pickering's papers (3).

1. All give the xanthoproteic reaction.

2. With copper sulphate and caustic potash, A gives a blue violet; B, *nil;* C, a typical violet coloration.

3. Their solutions do not coagulate on heating in the absence of salt; if, however, a trace of a soluble barium, strontium, or calcium salt is present, opalescence occurs at 56° and coagulation at 75° C.

4. The colloids are removed from solution (rising to the surface of the fluid) by saturation with magnesium sulphate,

ammonium sulphate, or sodium chloride. Here they especially resemble the class of proteids called globulins.

5. Another resemblance to globulins is seen in their behaviour to a stream of carbonic anhydride, which in the presence of salts causes precipitation. The passage of a current of air through the mixture redissolves the precipitate.

6. The colloid B is not digested by pepsin-hydrochloric acid; A is slightly digestible; but C is easily digested, and then the solution gives the typical peptone colour, pink, on the addition of copper sulphate and caustic potash.

7. Each of the colloids when intravenously injected into animals (rabbits, cats, dogs, rats, guinea-pigs) causes extensive intravascular coagulation. In a typical experiment death is due to respiratory failure, and 5 to 20 c.c. of a 1·5 per cent. solution is usually fatal. The other symptoms noticed are pronounced exophthalmos and dilatation of the pupil; in dogs there is often hyperpnœa immediately before death.

This last property of the proteid-like colloids is the most remarkable of the series, and its discovery is entirely due to Dr. Pickering. The resemblance to the action of the nucleo-proteids is most marked, and extends even to minor points, *e.g.*, neither cause intravascular clotting in the blood of albino rabbits; and in dogs very minute doses indeed, cause a slowing of the rate of coagulation; but for these and other details the reader must consult the original papers.

I need hardly say that the result was an unexpected one, and it by no means lessens the difficulties surrounding the coagulation question. So far as was previously known only nucleo-proteids produced intravenous clotting, with the single exception of snake poison. Snake poison, however, produces extensive disintegration of the vascular wall, and so it was considered that this was the source of the nucleo-proteid. The artificial colloids produce on the other hand little or no disintegration of leucocytes, and no injury to the capillary walls, so that the same explanation will not hold here.

If nucleo-proteids and these colloids both produce the same effect in the same way, one is driven to the conclusion that their physiological activity is connected in the first place with the heaviness of their molecules, and in the second with the presence of some radicle common to both. The colloid condition will not entirely explain the action, since many colloids do not act in the same way; the active radicle is certainly not one which contains phosphorus, since all the colloids are free from that element; it may possibly be the amido-fatty radicle in a high state of condensation which is responsible for the clotting produced.

It is these two principal sets of researches that I wished to bring before the readers of "SCIENCE PROGRESS," because although both fall short of their ultimate object, the synthesis of proteid, yet they show the way to be followed in the future, and, moreover, they exhibit in themselves certain points of interest, of which the one treated last, the physiological action of Grimaux's colloids, is by no means the least.

I have not alluded in the foregoing paper to Lilienfeld's work on the synthesis of peptone. He has only brought the matter forward in a preliminary notice, and for important researches of this kind, one requires full details before their value can be estimated.

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W. D. HALLIBURTON.

Science Progress.

No. 21. November, 1895. Vol. IV.

CARL LUDWIG, PROFESSOR OF PHYSIOLOGY IN THE UNIVERSITY OF LEIPZIG.

PART I.

LATE on the night of the 23rd of April there died at Leipzig the great physiologist and teacher, Carl Ludwig, one of the most distinguished savants of the present century. His name will ever remain a distinguished one on the bede-role of science, while the goodness, kindness and the singular amiability of his character will be cherished in the hearts of his numerous pupils, friends and admirers. Although the writer was personally very intimate with him, he feels strongly the difficulty of presenting a picture of the life, personality and work of Ludwig.

Carl Friedrich Wilhelm Ludwig, to give him his complete name, although he always signed simply C. Ludwig, was born on 29th December, 1816, on the banks of the Weser, in Witzenhausen, near Cassel, in Kurhessen. He attended the gymnasium in Hanau and subsequently studied medicine in Marburg and Erlangen, and became doctor of Medicine in Marburg in 1839 at the age of twenty-three. To the end of his days he bore the mark of a cut on his upper lip, a memento of his vivacious youth. In Marburg he became successively Prosector of Anatomy (1841), and Privat-docent of Physiology (1842), and Professor (Extraordinarius) of Comparative Anatomy in 1846. In 1849, he was called as Professor of Anatomy and Physiology to Zurich. From Vienna came an in-

vitation in 1855 to occupy the Chair of Zoology and Physiology in the so-called "Josephinum," which was a school for military surgeons, but which was afterwards abolished. There he remained for ten years, until towards the end of 1864 he was called to Leipzig, to which city he migrated in 1865 to become the successor of Ernst Heinrich Weber. At Leipzig Ludwig remained until his death. Here also for the first time he had an opportunity of building a new Institute of Physiology after his own plan, and here, during three decades, his extraordinary activity and his genius manifested themselves, so that soon his fame as an original investigator and teacher became known throughout the scientific world. The "Physiologische Anstalt" in the then Waisenhaus Strasse (now Liebig Strasse) became and remained for three decades perhaps the greatest centre of physiological activity in the world. Thither there flocked from all lands medical men who had already completed their curriculum, to study and prosecute research under this distinguished master. The "Anstalt" or "Institut" was constructed in the form of a large Roman E. From the centre of the vertical bar there projected the Lecture Theatre, so that all the rooms were thus well lighted. It consisted of three storeys. In the uppermost lived the professor and his family, and his faithful friend and mechanic Salvenmoser; in the lowest were the rooms for the laboratory servant, and numerous apartments for the gas-motor and other apparatus, and for animals, while the middle storey was the true workshop, and contained rooms for histological, chemical and experimental physiology.

When one talks of Ludwig's "pupils" one means more than the ordinary student of medicine who attended his lectures. What one means is this, *viz.*, that Ludwig not only taught medical students, but his pupils, or rather "young friends" as he called them, were medical men who, attracted by his fame as a teacher, came to his laboratory to pursue under his direction original research. In the laboratory during the session there might be ten to fourteen pupils, each pursuing an independent theme under his guidance.

Nearly all the younger generation of physiologists in Great Britain, America, Italy, Germany and Russia have at one time or another studied in his laboratory. The results were published annually in the famous and classical *Arbeiten aus der physiologischen Anstalt zu Leipzig* during the years 1866-77, *i.e.*, for eleven years (Jahrgang 1-11). At the foundation of Du Bois-Reymond's *Archiv* in 1877, Ludwig agreed to publish his work and that of his pupils in this journal, and so the independent issue of the *Arbeiten* as such came to an end. The results, however, were communicated to the *Sächische Akademie*, which in many cases contributed funds for the illustration of some of the monographs. Nearly 300 pupils studied under him, while the twenty volumes of *Arbeiten* represent much of the progress of physiology during the last three decades. To give an account of the work done by Ludwig and his pupils would in fact be to write the advances made in physiology during the last half-century.

To enumerate his continental pupils would be to name most of the professors of this subject, *e.g.*, Fick (Würzburg), C. Eckhard (Giessen), Kronecker (Berne), Drechsel (Berne), Gaule (Zurich), Lothar Meyer (Tübingen), Miescher (died August, 1895) of Basel, von Kries (Freiburg). In Russia, Setschenow, Woroschiloff, Tscheriew. In Sweden, Holmgren (Upsala), and Tigerstedt (Stockholm), Loven. In Belgium, Heger (Brussels), Lahousse (Ghent). In Italy, Mosso (Turin), Luciani (Rome), Fano (Florence), Baldi, Novi, and others.

Amongst his English and American pupils may be mentioned Lauder Brunton, Coats, Bowditch, Rutherford, Moseley, Ray Lankester, Stirling, Minot, Gaskell, Ward, Haycraft, Cash, Sewall, Meade-Smith, Wooldridge, Walton, Beevor, Buckmaster, Carslaw, F. S. Lee, F. Mall, W. H. Thompson and Vaughan Harley.

Ludwig's family life was one of the happiest that could be wished, and this, perhaps, partly because the circle was so small. He married in 1849, his only son died in infancy, while his only daughter became the wife of Alfred Dove, then the editor of *Im neuen Reich*, and now known as a dis-

tinguished historian and *litterateur*. His intimate personal friendship with many of the leading scientific and literary men of his time was to him a source of great joy. From the old Zurich days dated his intimate friendship with Th. Mommsen, in Leipzig. Apart from his warm friendship with his colleagues, more especially Professors His and Braune, he became the warm friend of G. Freytag, while to his intimate friends and compeers, the three great pupils of Johannes Müller, *viz.*, Helmholtz, Brücke, and Du Bois-Reymond, he dedicated his *Lehrbuch der Physiologie*.

During the long period of scientific activity he was the recipient of numerous honours and titles. He was a member of many learned societies, including the "Akademies" of Berlin, Vienna, Paris, Munich, St. Petersburg, Upsala, Stockholm, Rome, Turin. He was a foreign member of our own Royal Society, from which he received the Copley medal. An honour he valued greatly was that conferred on him by the citizens of Leipzig, in making him an honorary burgess of his adopted city. On the 15th October, 1874, Ludwig celebrated his silver jubilee as professor, on which occasion some of his pupils, as is the custom in Germany, presented him with a number of memoirs, which were published under the title *Beiträge zur Anatomie und Physiologie*, als Festgabe von seinen Schulern gewidmet. He was again the recipient of a similar honour on the occasion of his fiftieth year as doctor. During his last illness, and when I went to Leipzig in the hope of seeing him, von Frey reported that his strength was well maintained. At that time it was proposed to have his portrait painted and presented to him on his eightieth birthday, as a small token of the esteem in which he was held by his pupils, but, alas! it was not to be. He died while the arrangements were being made.

Ludwig devoted himself with the utmost assiduity and with untiring constancy to the work of his laboratory, but from time to time he sought relief from the intense strain of steady application to work by short journeys in Germany, Italy, Belgium, Holland and England. For him the natural beauty of the country had a great fascination, while art in

every form was a source of pleasure. How well he could discourse on the beauties of Italian art, on the gems of Van Eyk and the etchings of Dürer was well known to some of us who had the inestimable pleasure of free intercourse with him. Up to the very end of his life he preserved a wonderful freshness and elasticity. In April, 1894, when I last saw him, he seemed to have changed but little in appearance, whilst the same interest in everything new in the scientific world awakened his eager interest. The only sign of age he then manifested was his fear that he was fast becoming old, and that he no longer possessed in the fullest manner the power of doing his duty as he wished to do it. He continued to lecture without discomfort during the session 1894-95, nay, he even conducted the examinations for the degree at the end of the session, when towards the end of March, 1895, he was seized with an attack of influenza, and just when he seemed to be recovering he was suddenly seized with heart failure, late on the night of the 23rd of April, at 11·30 P.M. As he raised himself to take a drink he fell back in bed; and thus passed away in the fulness of years and full of honour, perhaps—nay, the greatest teacher of physiology of this or any century. Although for seven weeks he was confined to his bed, yet he suffered little pain; and at his grave it was said of him as of Moses: "Seine Augen waren nicht dunkel geworden, und seine Kraft war nicht verfallen," so that mentally he remained "young and fresh" to the last.

His love for morphological studies was very marked. He had always a microscope beside him, and whenever he had a spare hour he turned his attention to histology. During the last ten years many beautifully illustrated memoirs were published by the Saxon *Akademie*, dealing more especially with the distribution of blood-vessels. The last of these, the arrangement of the blood-vessels of the inner ear, more especially of the semicircular canals, was being produced by his pupil Eicher, but he died ere the work was finished, and so Ludwig finished it shortly before his own death. One of the earliest and most important anatomical memoirs done under his direction was

by Leber, *On the Blood-vessels of the Eyeball.* Most students are familiar with reproductions from this work in the text-books. His microscopical preparations were always made with the greatest care, and when a drawing of a preparation had to be made it had to be done exactly, and with an artistic touch. He always insisted on having the best draughtsman available. His fine artistic feeling led him to this, and surely had he lived in the times of Vesalius, he, like Vesalius, would have had Titian and his pupils to design his plates. To Ludwig, as to Spallanzani, nature was as a large picture, the most remarkable, charming, and fascinating which could be presented to human view, and great was his joy when he was able to lift the veil from some part of the picture and disclose to view some of its hidden mysteries. Of his pupils who have become distinguished anatomists or histologists we may mention Schwalbe, Froriep, Braune, Flechsig, Krause, Minot and many others.

Physiology is at once an ancient and a comparatively modern science. Haller, the greatest physiologist of last century—and in whose honour Kronecker has named the magnificent new Physiological Institute at Berne, Hallerianum—called it "animated anatomy". Our definite knowledge depends largely on the progress made during the last fifty years, and to this progress Ludwig either directly or indirectly has been one of the most potent contributors. Indeed there is scarcely a chapter in physiology on which he has not left the impress of his genius, and which he has not enriched with new facts. It was indeed a great good fortune that Ludwig dedicated his life to the advancement of physiology. The circumstances were propitious, but even granting this, the form in which the study of the subject presents itself at present is largely due to his activity, to his discoveries, to his method of experimenting, and to the original manner in which he trained numerous young investigators to follow in his footsteps.

From a literary point of view the contributions made by Ludwig in his own name are not numerous, but such

as he chose to issue over the simple signature of C. Ludwig have become classical. His life-work finds expression in the long series of scientific memoirs, which contain the result of his investigations for the most part undertaken along with his pupils, while the memoir itself usually bore only the pupil's name. He published the second edition of his *Lehrbuch der Physiologie* in 1858-61, dedicating it to his three friends, Du Bois-Reymond, Helmholtz and Brücke. The book opens with the striking words, not so striking now perhaps as they were then, when physiology was largely dominated by the so-called vitalistic doctrines of Johannes Müller: "Die wissenschaftliche Physiologie hat die Aufgabe, die Leistungen des Thierleibes festzustellen und sie aus elementaren Bedingungen desselben mit Nothwendigkeit herzuleiten," and his aim was to show that "alle vom thierischen Körper ausgehenden Leistungen eine Folge der einfachen Anziehungen und Abstossungen sind". In other words, he sought to bring the phenomena manifested by living beings into line with those of physical science, and to apply to their elucidation the same methods as are applicable to the study of physical and chemical phenomena. To study or even to regard biological phenomena from a physical and chemical standpoint was at that time still a new conception, yet this idea, as v. Kries well expresses it, formed the "Grundton" of Ludwig's whole scientific bias. Indeed, his first scientific work on the secretion of urine was carried out on these lines, and to the end he remained true to this principle. But Ludwig was well aware that there are many phenomena manifested by living organisms for which there are no directly analogous phenomena in inorganic nature; but the point is, that his endeavour throughout was by anatomical, physical and chemical investigations to arrive at such a knowledge of the organ to be investigated as to secure a basis on which the interpretation of the phenomena manifested by it might become explicable by facts already known. One of the necessities of his whole conception of any physiological process was to obtain a comprehensive and palpable representation of the relation between an

organ and its function. With his striving after physical exactitude, there was no fear of his taking too narrow a view of the activities of an organ, *i.e.*, as a merely physical apparatus. This one may illustrate by his researches on muscle, which he regarded not merely as a physical apparatus; he took a far wider and more comprehensive view, he regarded it as an organ whose histological structure had to be investigated, and its physical characters studied; but even that was not enough, it had to be studied from the point of view of how it comported itself when its blood-vessels were perfused with blood, how its properties changed as its nutrition altered, how it breathed, and how its activities were affected by nervous and other influences. Thus we must give a wide interpretation to his views of the physical and chemical processes accompanying the manifestation of so-called vital phenomena.

One very characteristic aspect of his work lay in his strong morphological bias. The anatomical aspect of a problem always attracted him greatly, and he always had on hand some anatomical and histological investigation from which he derived new inspiration for future physiological research. The distribution of the blood-vessels in various parts of the body had for him a peculiar fascination, their relation to other tissues was ever present to his mind. As a consequence we have the beautiful researches on the origin of lymphatics; the effects of the respiratory movements on the movements of lymph; his theory of the mechanical results dependent on the distribution of the capillaries in the glomeruli of the kidney, and how this affects the passage of fluids from the renal vessels into the urinary tubules; how the blood-vessels behave in contracted and relaxed muscle. To what important discoveries he was led by a study of the arrangement of the heart muscle fibres we shall see presently.

Dealing first with the researches published over his own name, we have the classical paper "Beiträge zur Lehre von Mechanismus der Harnsecretion," which he presented to the Faculty in Marburg in 1842, when he qualified as "Privat-docent" of physiology. In this con-

tribution, after studying the minute structure of the kidney, more especially the distribution of its blood-vessels, and after numerous experiments, he attempted to explain the process of urinary secretion on a physical basis. With his pupil Goll, still later, he showed the dependence of urinary secretion on blood pressure, and hence arose the so-called "filtration hypothesis". In 1844 in *Wagner's Handwörterbuch*, vol. ii., p. 628, his researches, extending to twelve pages, are given under the title "Nieren und Harnbreitung". Just as it was his earliest work, so a study of urinary secretion cropped up every now and again in his laboratory. Ustimowitsch in 1870 showed how curare diminished urinary secretion, how the secretion was influenced by section of the renal nerves, and above all, from the more modern standpoint regarding the secretion of urine, this other important fact that urea when injected into the blood-vessels—the blood pressure being very low, so low, indeed, that no secretion of urine took place —again caused a secretion of urine. This fact, recorded by Ludwig's pupil, is to-day utilised to explain secretion by the presence of "harnfähigen" substances in the blood. In 1893 Grijus showed that "the kidneys produce heat in proportion to the water they excrete"; and again we have the important fact brought out in this paper from a comparative study of muscle, salivary gland, and kidney, that while muscle and salivary glands derive their impulse to the production of heat from nerves, in the case of the kidney this is not so; the kidneys are excited to excrete only when the blood is charged with "harnfähigen" substances. But while this is so, one of the contributions which made him famous was his investigation on the influence of the nervous system on glandular activity. By his classical researches on the submaxillary gland, "Neuen Versuche über d. Beihilfe d. Nerven z. Speichelabsonderung," Ludwig completely retransformed the theory of salivary secretion, and of secretion in general. He showed that secretion could no longer be regarded merely as a process of filtration; on the contrary, it was associated with chemical and thermal changes in the gland and in the

blood streaming through it. Nay, he showed that secretion might take place in a decapitated animal, provided the nerve going to this gland be stimulated. Numbers of his pupils continued to work at salivary secretion, both from its physiological aspect, *e.g.*, Becher, Rahn, Spiess, and its histological.

Indeed Ludwig's contributions to the physiology of the excretions may be regarded as amongst his most important contributions. In connection with the salivary glands, his histological investigation paved the way for important experimental results. With Gianuzzi he investigated the structure of the salivary glands, and in his celebrated paper, "Ueber den Einfluss d. Beschleunigung d. Blutsstromes auf die Absonderung des Speichels," Gianuzzi discovered those curious bodies in the mucous salivary glands which still bear his name and are called "demilunes" or "Gianuzzi's crescents". There are numerous and classical investigations on the secretion of saliva. Indeed so late as 1889 we find Novi investigating "Die Scheidekraft d. Unterkiefer drüse". But while Ludwig's investigations not only completely transformed the theory of secretion, by his investigations he added an entirely new class of nerves to those already known, *viz.*, those known now as "secretory nerves".

Up to the time that Ludwig stimulated the nerves to the salivary glands, physiologists were acquainted only with contraction of muscle resulting from stimulation of the peripheral end of a divided nerve. That the nervous system affected the secretions and excretions had been known from time immemorial—for example, the flow of tears under emotion, the arrest of salivary secretion under the influence of fear—but these results might be due to the supply of blood or other cause. When Ludwig began his researches, all these and other secretions were regarded merely as due to simple filtration of fluids through different membranes. Ludwig proved that this is not so, but that the nerves act directly upon the gland cells themselves, and thus regulate the chemical metabolic processes going on within the secretory cells. Ludwig's great contemporary, Claude Bernard, discovered the "vaso-motor nerves," or those nerves

which influence the calibre more especially of the small arteries. Could the physiology of salivary secretion be explained by this new mechanism? Ludwig by ingenious experiments soon showed that in salivary secretion there was another influence at work beside the action of vaso-motor nerves, *viz.*, the action of glandular or secretory nerves, so that one of his great discoveries was the existence of a separate class of nerves now known as "secretory nerves".

But perhaps Ludwig's greatest interest was the doctrine of the source and movements of the various juices of the body, more especially of the blood and lymph, and here we have to deal with an amazing number of contributions. Indeed, in no other laboratory have the phenomena connected with the movements of these fluids been so thoroughly investigated as in that of Ludwig. The arrangement of the muscular fibres of the heart early attracted his attention, but to him this meant more than merely to describe their arrangement. He sought to explain this arrangement, and how it was useful to the heart to enable it to empty itself, why also the right ventricle forms a crescent-like cavity round the left ventricle, while by a study of the arrangement of the fibres around the auriculo-ventricular orifices he showed how important it was from the point of view of preventing the reflux of blood into the auricles during systole that these orifices contracted, thus affording at the same time a better *point d'appui* for the segments of the auriculo-ventricular valves, the arterial orifices at the same time remaining unchanged in their dimensions.

Ludwig's name, however, will remain indissolubly connected with his epoch-making invention of his "Kymograph" or "Kymographion" or "Wave-writer," an instrument to be found in all laboratories. The importance of this invention is twofold: first, on account of its practical value as a means of measuring the pressure exerted by the blood upon the inner surface of the larger blood-vessels, *i.e.*, the blood pressure, but second, because by it Ludwig founded the graphic method in physiology, a method

which had previously been adopted for other purposes by our distinguished countryman, Thomas Young, but which had been forgotten, so that Ludwig not only founded the graphic method, but gave it back in a greatly improved form both to physics and meteorology.

When an artery is severed, as every one knows, the blood comes out in jets, a proof that the pressure within the arteries exceeds the atmospheric pressure. The Rev. Stephen Hales, Rector of Teddington, described in 1727 in his *Statical Essays* how he introduced the end of a long vertically placed tube, seven to eight feet in height, into the femoral artery of a mare, and observed how high the blood mounted in this tube. This it did to a height of nearly seven feet, so that this represented the "blood pressure". In 1828, Poiseuille, a French physician, connected a blood-vessel of an animal with a U shaped glass tube, or manometer, partly filled with mercury, one end being connected to the artery by means of a lead tube filled with carbonate of soda solution to prevent the blood from coagulating. Thus any oscillations of the mercury could be read off in the open limb of the tube. This was a great advance on the method of Hales, but still it was unsatisfactory. Ludwig, in 1847, modified this instrument by placing a light swimmer in the open limb of the tube and causing the swimmer to record its movements on a revolving surface moved by clockwork. By this method a permanent graphic record was obtained, a curve whose height was an expression of the blood pressure within the artery, and its extent a measure of the time of the several phases of the curve. For the first time the pulsations of the living heart were recorded on paper.

The story of the Kymograph is an interesting one. In 1846, while Ludwig was still in Marburg, he studied the relation between the respiratory movements and the blood pressure, and on his Kymographion he recorded the movements of respiration and the variations of the blood pressure simultaneously. As it were by inspiration the graphic method was founded. For the first time on the same piece of paper was recorded the respiratory movements of the

chest-wall, the rapid movements of the heart, and the blood pressure. Thus was opened up a new and fertile field of study. Since then in all sciences the graphic method has played an essential part. On its applications to physiology, Marey, Professor in the Collège de France at Paris, has written a whole book, and he himself has greatly extended our knowledge of technical appliances for this purpose. Ludwig published his results in Müller's *Archiv*, pp. 240-302, 1847, "Beiträge zur Kenntniss d. Einflusses d. Respirations-Bewegungen a. d. Blutlauf im Aortensystem".

To a dearly loved and much esteemed pupil, Professor Angelo Mosso of Turin, Ludwig presented the original tracing which was published in Müller's *Archiv* with the characteristic dedication on the back of the tracing, written in the neat crisp characters for which Ludwig was famous, "Der Sammlung des Freundes Mosso stiftet dieses erste Stammeln des Herzens und der Brust. C. Ludwig, Leipzig, 15th August, 1874." On the front of the tracing was the date xii., 1846. It is obvious that the Kymograph, apart from its intrinsic importance in the laboratory, is the forerunner of the innumerable applications of the graphic method. To record one tithe of the results won by this instrument or its descendants would be to record the progress of the physiology of the vascular system during the last half-century.

Confining our attention for the present to the heart, in vol. i. of the *Arbeiten*, issued in 1867, we have a description of a very remarkable nerve. In the neck of the rabbit and some other animals there is a small isolated nerve, a branch of the vagus nerve in the neck, which pursues an independent course and runs to the heart, or, speaking physiologically, proceeds from the heart, to join the vagus, and is carried above the larynx in this nerve-trunk to join the medulla oblongata or bulb. This nerve was discovered by Ludwig and Cyon, and was named by them the "depressor" nerve. Why? They found that when this nerve was divided and its *central* end stimulated, the blood pressure in an artery being recorded simultaneously by means of the Kymograph just described, after a few seconds there was a

steady fall of the blood pressure within the arterial system. Here we have a very remarkable phenomenon, how is it to be explained? Well, this nerve is an afferent nerve from the heart, *i.e.*, it conducts impulses from the heart to a part of the central nervous system or bulb, which, when they reach that organ, so modify the activity of the so-called vaso-motor centre situated therein, as to permit of the blood-vessels of the abdomen becoming so dilated that so much blood accumulates within them as to leave only a limited amount for the general circulation, and hence the steady fall of the blood pressure. But what is the use of this nerve? Ludwig suggested that by means of it the resistance offered to the heart, the essential motor-factor in the circulation, was regulated. In other words, when there is too great resistance to the heart, so that it cannot with ease discharge its contents, the terminations of this nerve in the heart are excited and thus lead to a dilatation of the abdominal vessels, whereby the resistance in the arteries is diminished. Thus through this nerve the heart —within limits—can regulate the resistance offered to the blood discharged from it.

This nerve is "depressor," therefore, because when stimulated it brings about a diminution of the pressure within the systemic arteries. It is, however, also the sensory nerve of the heart, and is, probably, one of the nerves concerned in the excessively painful attacks which characterise "angina pectoris" or breast-pang. At the present time this nerve may have a special interest apart from its physiological significance. Few of our readers will, perhaps, recognise in the co-discoverer of this nerve, and one of the earliest pupils of Ludwig, a man who has played many parts in the world—physiological and political. Cyon, as his career has proved, was what the Germans call a many-sided man. The writer made his acquaintance in Leipzig many years ago, when he was Professor of Physiology in St. Petersburg, and, if he is not mistaken, the M. de Cyon who a few weeks ago was deprived of his rights as a Russian citizen by an Imperial ukase is E. Cyon of depressor nerve fame. Many years

ago Cyon left St. Petersburg, and devoted himself to journalistic work in Paris.

Purposely not taking Ludwig's discoveries in chronological order, let us turn for a moment to another nerve in connection with the heart, whose action he discovered, *viz.*, the nerve which from its action he called "Accelerans Cordis". This nerve passes from the upper dorsal region of the spinal cord to the heart, and when it is divided and its peripheral end stimulated, it causes a great acceleration in the number of heart beats, hence the name given to it by Ludwig and Baxt. In this country it is also called Augmentor cordis. Naturally, Ludwig compared the effect of the vagus on the heart, discovered by his predecessor E. H. Weber, with that of the accelerans. The vagus or its inferior cardiac branch when stimulated slows or may even arrest the action of the heart in the relaxed or diastolic phase for a few seconds. On comparing the results obtained by simultaneous stimulation of these two nerves, Ludwig and Baxt came to the conclusion that they do not act on the same intracardiac mechanism.

But while the Kymograph enabled Ludwig and his pupils to discover numerous important facts relative to the action of the heart, it primarily put one in possession of facts which led to an entirely new conception of the activity of the blood-vessels or at least the action of the nervous system thereon. Ludwig and Thiry, in 1864, showed the influence of the cervical spinal cord on the blood stream, dwelling particularly on the fact that the varying and variable capacity of the portal, *i.e.*, abdominal, area was largely concerned in the regulation of the blood pressure in the branches of the aorta, a fact which fits in well with the subsequent discovery of the depressor nerve.

In the medulla oblongata or bulb, Ludwig and his pupils, Owsjannikow and C. Dittmar, succeeded in determining accurately the position of the so-called chief vasomotor centre, *i.e.*, a part of the nervous system from which impulses are continually proceeding to the smooth muscular fibres, more especially of the small arteries or arterioles, whereby these muscular fibres are kept in a state of partial

contraction. The effect on the blood-flow, and, therefore, on the nutrition of the part supplied by such an arteriole, is obvious. This partial contraction is called vascular tonus.

Ludwig and his pupils mapped out accurately the position of the vaso-motor centre. To Claude Bernard belongs the merit of the discovery of the nerves which influence the calibre of blood-vessels. On dividing the sympathetic nerve in the neck of a rabbit he found that the blood-vessels of the ear and face on that side flushed or rather blushed, while stimulation of the distal end of the nerve caused pallor of the corresponding parts.

Perhaps one of the most important methods introduced by Ludwig, but which grew gradually from small yet important beginnings, is that of the nutrition of excised organs. This method consists in circulating or perfusing blood or other fluid through the blood-vessels of an excised or surviving organ, *i.e.*, when the excised organ possesses blood-vessels. The fluid perfused may be blood or other fluid. The method had its origin in the experiments conducted by Cyon and also by Coats on the "isolated," *i.e.*, "excised," frog's heart, and gradually it grew in importance until it came to be one of the most important methods used in studying the localised action of drugs.

This method depends on the fact that all our tissues or organs do not die simultaneously. After so-called death of the individual, the separate organs, especially in cold-blooded animals, may retain their vitality for many hours or even days. The heart of a frog or tortoise when cut out of the body will beat under appropriate conditions for one or two days. By means of this method the chemical and other conditions necessary for cardiac activity were studied. In studying the action of "surviving excised organs," *e.g.*, the frog's heart placed in connection with a recording manometer, H. P. Bowditch, Professor of Physiology in Harvard University, Boston (U.S.), showed that the muscle of a frog's heart, quite independently of the strength of the stimulus applied to it, either gave a maximal contraction or it did not contract at all. On this A. Mosso, with that Southern poetic spirit which characterises his way of look-

ing at physiological facts, remarks "that the heart is at once the most delicate and strongest of all our organs, because when excited by weak stimuli, it always responds with a strong contraction—in fact, with its strongest possible response" (*Die Nation*, 29th June, 1895).

As regards the heart and the cause of its rhythm, quite a number of the older theories regarding its rhythmical action have been put aside, largely owing to the results obtained by this method. It is, at least, something to get rid of false theories.

Keeping still to this method of perfusion, it has had a far-reaching influence. The writer can well remember with what diligence Luciani, now Professor of Physiology in Rome, perfused blood through the excised heart of a frog, and studied the remarkable groups of beats, with intervening pauses, which bear his name of "Luciani's groups".

In the opinion of the writer about this time there commenced a movement of young men from Italy and Leipzig which has exerted a great, not to say commanding, influence on the physiological achievements of our Italian *confrères*.

Under the guidance of Kronecker, then the assistant of Ludwig in the experimental department, Luciani studied with patience the remarkable periodicity in the action of the frog's heart whereby groups of beats alternate with periods of repose.

The artificial circulation of blood through excised organs was obviously capable of great expansion. Ludwig proposed to A. Mosso to study artificial circulation in an excised kidney, and the writer remembers what lively interest was awakened in the laboratory by the earliest experiments, *i.e.*, to study the life of a kidney after death. It was soon discovered that even twenty-four hours after the kidney was excised from the body, this organ still lived or survived, or at least that its blood-vessels did, and responded to the action of drugs.

From these investigations originated the idea suggested by Ludwig to Mosso of measuring the volume of an organ due to variations in the size of its blood-vessels. Mosso

arranged an apparatus by means of which variations in the volume of a limb or other organ might be easily recorded whilst still in connection with the living animal. This he called a plethysmograph. Very many investigations have been made by it. By means of it one can show that the volume of the arm, or a limb, varies with the activity of the brain, and over and over again has the writer seen the effect of a mental effort—*e.g.*, an arithmetical question while being solved by the person experimented on—varying the amount of blood supplied to the arm, and therefore the volume of the limb. Many other Italian pupils flocked to Leipzig—for Ludwig had always profound sympathy with his Italian "young friends". Perhaps it will not be taken amiss by his Italian *confrères* and friends if the writer ventures to express his opinion that the extraordinary activity in physiological circles manifested by the younger Italian pyhsiologists is largely if not directly due to the inspiration of Ludwig.

As Italian pupils, we may mention Ceradini (1871), perhaps one of the most talented amongst his compatriots. Alas, that Ceradini should have turned his attention to other affairs! His research on the semi-lunar valves is one of the best monographs on this subject, and still remains a classical memory. Gianuzzi, who has given his name to the "crescents" which occur in the acini of certain mucous salivary glands, and then followed Luciani, Mosso, Gaglio, Baldi, Grandis, Fano, Martinotti, and many other well-known Italian investigators.

By a comparatively simple method Ludwig showed the connection between serous cavities such as the peritoneum and the lymphatics, and demonstrated how the respiratory movements aid in the passage of lymph and even particulate bodies from the peritoneal and pleural cavities into the lymphatics *viâ* the stomata. With his first assistant in his histological department, Schweigger Seidel, who died early, he invented the now familiar method of injecting lymphatics by the "Einstich" or "Puncture" method. Brücke had discovered the value of soluble Berlin blue as an injection mass, but it remained for Ludwig, reasoning from the

presence of the valves in the lymphatics, to conclude that the natural path for the lymph and therefore other fluids to pass in these vessels was from periphery to centre, and so he hit upon the apparently simple yet really scientific method of injecting these vessels with a watery solution of Berlin blue, the nozzle of a fine hypodermic syringe being thrust under or into the skin or other organ, when lymphatics are immediately filled with the blue fluid. Indeed it is a most beautiful and instructive experiment to show the lymphatics in the limb of a dead animal by this method. In this way these observers were able to study the distribution of lymphatics in fasciæ and tendons, and to show how important are movements for the onward flow of the lymph, a view that was confirmed by the experiments of Genersich, Lesser, Emminghaus, and others on the effect of movements —active and passive—on lymph flow. Ludwig was continually striving to arrive at a correct knowledge of the relation of the tissue elements to the lymphatics on the one hand, and the blood-vessels on the other. Numerous experiments led him to formulate the filtration hypothesis regarding the formation of lymph, an hypothesis which is still *sub judice*, and is even to-day the subject of lively controversy. Whatever theory be adopted Ludwig's facts remain.

During the last ten or twelve years, Ludwig's pupils experimented largely on the process of Nutrition as manifested in the absorption and fate of proteids, sugars and fats. Numerous are the investigations on the course of the fat on its way to the blood. With what care and exactness did Ludwig operate when exposing the termination of the thoracic duct! Indeed he was an ideal operator, and had he chosen to turn his attention to surgery few would have equalled him in the art. The passage of proteid and sugar into the blood was the subject of many investigations, and so was the effect of the injection of so-called peptones into the blood stream (Schmidt-Mulheim and Fano) whereby the blood of the dog when it was shed was prevented from coagulating. Correlated with this are the numerous investigations on the respiratory processes involving a study of the gaseous exchanges in the lungs and the tissues.

To Ludwig we owe our earliest exact knowledge of the blood gases, and to this end he devised much apparatus which is in daily use in physiological laboratories. A question which always had great interest for him was the respiration of the tissues, whether oxidation took place in the blood or in the tissues, a question now answered in the affirmative for the tissues. There are many researches on the physiology of muscle, including the famous work of Kronecker on its fatigue and recovery.

The physiology of the spinal cord and the doctrine of reflex action was thoroughly studied, both in the frog and mammal. Our knowledge of the sensory paths in the cord was greatly extended by the researches carried on in his laboratory, while Ludwig himself devised a whole series of delicate instruments for section of limited parts of the cord. It was when Flechsig was his assistant in the histological department that, under Ludwig's inspiration, he made those fundamental investigations on the medullation of nerve-fibres, which enabled him to map out new conducting paths in the cord. This research at once established Flechsig's fame, and led to his appointment to the Professorship of Psychology in Leipzig. Indeed by a curious coincidence Flechsig was Rector of the University at his master's death. In his speech at Ludwig's grave as Rector, Flechsig remarked that "Ludwig was not only a high priest of his science, but of science in general".

There is not a large number of investigations on the sense-organs, while on the brain or rather cerebrum and cerebellum I do not recollect to have seen any independent investigations.

Numerous investigations of a more or less chemical nature were carried on, for obviously experimental and chemical physiology must frequently go hand in hand, and in this respect Ludwig was fortunate in having Drechsel as his assistant, and in later years Siegfried. Another of Ludwig's pupils whose contributions belong to the borderland between chemistry and pharmacology is Schmiedeberg, the distinguished and genial Professor of Pharmacology in Strassburg.

Ludwig always selected his assistants with great judgment and discrimination. In the histological department, he had successively Schweigger Seidel, Schwalbe, Flechsig, Gaule, Drasch and Moser. In the physical or as he called it experimental department, Kronecker, von Kries and von Frey, while Drechsel was his chemical assistant until called to Berne as Professor of Physiological Chemistry. Those of us who had the great good fortune to study under Ludwig when Hugo Kronecker, now Professor of Physiology in Berne, was assistant, can never forget how much we owe to Kronecker, who was a second *genius loci* and the fast friend and helper of us all. He it was who led the way to the foundation of the then "physiologische Gesellschaft" in Leipzig, and who was the originator and presiding genius at the "functions" that followed after the formal communication of "papers" at the evening meeting of the society. But Ludwig's personal assistant, in the narrower sense of the word, was his mechanic Salvenmoser, who accompanied him from Vienna. He was simply invaluable both to professor and pupil, and he always did his work so thoroughly, yet quietly. With the death of his master Salvenmoser has retired, partly on a pension or, rather, on an annuity subscribed for him several years ago by Ludwig's pupils.

One other feature deserves notice, *viz.*, Ludwig's care for animals operated on. He lays it down explicitly that an operation on a living animal should only be undertaken after careful forethought of all details of the experiment, and after the animal has been rendered insensible by the use of some anæsthetic. He would never allow an animal to be used for a second operation. In 1879-80 he was President of the Society for the Protection of Animals in Leipzig, and remained a member of this society until his death.

Such is a rapid sketch of the work which was conducted under Ludwig's direction, and it will be obvious that Ludwig was an "all-round" physiologist, and perhaps the only one who could be compared with him in the breadth and variety of his knowledge was his former colleague and friend, Brücke,

who, like Ludwig, was also deeply imbued with the artistic sense.

It is, perhaps, not too much to say that Ludwig's labours have largely revolutionised physiology, while our whole manner of looking at physiological problems is largely due to the influence he has exerted on the progress of the science. He was essentially, and above all, a born experimenter. He was thoroughly at home both in the laboratory and lecture room. His views he always submitted to experimental investigation, and in doing so he exhibited a faculty for the invention of mechanical appliances which was surprising. One, perhaps, of the best known and most ingenious of these is his method—along with Dogiel—of estimating the velocity of blood-flow by means of his "Stromuhr," an instrument well known to students of physiology. Ludwig was, in the truest and widest sense of the word, not only a physiologist but also a philosopher. It was not enough for him to "speculate' or "theorise" upon a subject, the theory had to be submitted to experimental proof.

Moreover many departments of physiology which had long remained within the sphere of vitalistic influence were by him submitted to investigation by the most exact physical methods. In fact Ludwig did for physiology as a whole, what Spallanzani did for a great chapter of chemical physiology. Abbé and professor were imbued with the same spirit.

The preceding pages are to be regarded as a slight tribute of admiration and gratitude, and as a thankoffering to C. Ludwig, that "homo ammirabile" as Fano calls him, from one who received much kindness and instruction at his hands, and who had the honour and privilege of much intercourse with him for many years. Whatever monument may be raised to his memory by his pupils, his works and the imperishable impress which his genius and his labour have imprinted on physiology will ever remain his true monument, to attest the greatness of his intellect to succeeding generations.

WILLIAM STIRLING.

ON THE CHEMICAL NATURE OF ALLOYS.

IN the preceding article I assumed for the sake of simplicity that no chemical action takes place between the metals of an alloy. The evidence on this point is very imperfect, but in the majority of cases the balance of probability is in favour of this view. Some metals, however, undeniably combine in definite proportions to form chemical compounds.

It has been already mentioned that the depression in the freezing point of a solvent is proportional to the number of molecular weights of foreign substance dissolved in it. Moreover, the depression predicted by Van't Hoff's law as due to one *molecular* weight, is in many cases produced by one *atomic* weight of a metal when dissolved in other metals. Hence it seems probable that a metal is separated into individual atoms during the process of solution. It may be, however, that molecules are formed containing an atom of the dissolved body associated with one or more atoms of the solvent. Where the depression in the freezing point is persistently one half the theoretical, as in the case of aluminium dissolved in tin and of mercury or bismuth in lead, it becomes, at least, probable that the molecules in solution are Al_2, Hg_2, Bi_2 respectively. Tin in lead appears to form molecules of the type Sn_4.

In these cases we do not know how much, if any, of the solvent is associated with the molecules of the dissolved metal. In fact the cryoscopic study of binary alloys throws very little light on the question of chemical combination; but when we examine ternary alloys we get more evidence on this point.[1] For, as a general rule, when two metals B and C are simultaneously dissolved in a third, the total depression in the freezing point is the sum of the effects that B and C separately would produce. This fact indicates that the atoms of B and C have not combined with each

[1] Heycock and Neville, *Chem. Soc. Jour.*, 1891, p. 936, and 1894, p. 65.

other, for such combination would decrease the number of molecules in solution, and consequently diminish the depression in the freezing point. When, however, one of the metals tin, bismuth, thallium or lead is the solvent, and gold and cadmium are added in succession, we obtain evidence of combination between the two latter. For if the gold be present in sufficient quantity, and the cadmium be now added, then, instead of causing a further depression in the freezing point, this latter metal will bring about a rise. Successive additions of small quantities of cadmium will produce alloys whose freezing point gets higher and higher until an equal number of atomic weights of cadmium and gold is present. With such a proportion, the alloy has a maximum freezing point, which is still, however, lower than that of the pure solvent. This maximum freezing point is quite independent of the amount of gold and cadmium present, so long as this exceeds a certain value, and the two metals are in equal atomic proportions; but an addition of either gold or cadmium beyond this proportion produces a further fall.

These phenomena point to the conclusion that the gold and cadmium have partially combined to form molecules, AuCd, of a body which is less soluble than either metal. This compound has been separated from the mother liquor in a somewhat impure form by a process of filtration; it has also been prepared in a purer form by alloying gold with cadmium and removing the excess of the latter metal by distillation in vacuo.

When aluminium is dissolved in tin and gold is added a quite insoluble body $AuAl_2$ is precipitated. Gold with zinc, and silver with cadmium or zinc, probably form similar definite compounds, but they have not been so thoroughly studied.

If the above view as to the nature of these alloys be correct, we see a selective power of combination with each other shown by the two metals in solution. The solvent metal, though present in overwhelmingly larger proportion, does not appear to appropriate either of the others; but the evidence we possess does not tell us whether the molecule

of the compound in solution contains atoms of the solvent metal or not. All we can say is, that the gold and the cadmium behave in the same way to each other whether the solvent metal be tin, thallium, bismuth or lead. This fact makes it probable that the chemical influence of the solvent is a subordinate one as compared to the action of the two metals on each other.

It is noteworthy that the compounds of gold or silver with cadmium do not appear able to exist in solution except in the presence of excess of the constituent metals. In this respect they resemble many chemical compounds that partially dissociate in solution. We can arrive at this conclusion in the following way. The total depression in the freezing point of the alloy below that of the pure solvent gives us, by Van't Hoff's law, the total number of molecules in solution, irrespectively of whether these molecules are complex or simple. But we know the weights of gold and cadmium that have been added, and combining these several data we can calculate the concentrations, x, y, z, of free gold, free cadmium, and of the compound AuCd which are in solution. The calculated values show that although x, y, and z vary very much for different alloys, yet they are connected by the law usual in cases of dissociation, namely, that when x is large y is small, and *vice versâ*; or, more exactly, that their values obey the law of mass action $xy = kz$, where k is a constant. The compound of gold and aluminium is more stable, and does not dissociate when in solution in tin.

There can be no doubt that further cryoscopic examination of ternary alloys would bring to light numerous cases similar to the above.

Another method of research has been adopted by Laurie,[1] who has studied the copper-zinc, copper-tin, and gold-tin alloys, and as the methods and results are similar in the three cases, we will consider the experiments on copper-tin. He prepares a series of alloys containing increasing percentages of tin, and makes each alloy in turn one plate of

[1] *Chem. Soc. Jour.*, l., p. 104, 1888; and *Phil. Mag.* [5], 33, p. 94.

a voltaic cell, the other plate being copper, and the electrolyte being a solution of stannous chloride in hydrochloric acid. The electromotive force of each combination is then determined and the results plotted. The curve thus obtained shows that so long as the alloy contains less than 40 per cent. of tin, the electromotive force is small, somewhat irregular, and does not vary much from ·1 volts, but that as soon as this percentage is exceeded, it runs up to a much larger and very constant value of ·56 volts, a value which is independent of the excess of tin present. The change probably occurs with a slightly smaller percentage of tin, very near the composition Cu 61·64 per cent., which corresponds to the formula Cu_3Sn. He considers that the change is due to the first appearance of crystals of free tin in the solid alloy, the tin when in less quantity being all in combination with the copper as Cu_3Sn. Of the other body Cu_4Sn, which Riche believes to exist, he found no indication.

In the copper-zinc alloys a similar change in electromotive force was found to occur when the composition $CuZn_2$ was reached, and in the gold-tin series at about AuSn. These experiments belong to a large class, including those of Matthiessen, Oliver Lodge, Wiedemann, and others on electrical conductivity, those of Roberts Austen with the induction balance, and numerous experiments on the density of alloys. In all these the physical properties of a series of alloys have been shown to experience an abrupt change at a particular composition. That these facts point to real and important differences between the alloys on either side of the critical percentage is certain, but it would be premature to conclude that their origin lies in chemical combination. Other explanations certainly can be offered. For example, it has been suggested that the much finer grain in the crystalline structure of alloys between certain percentages might prevent the electrolyte from attacking the more positive metal except superficially, and so cause the electromotive force after a very short time to become that of the more negative metal.[1]

[1] Foerster, *Naturwissenschaftliche Rundschau*, ix., 36.

Mylius and Fromm[1] have made an interesting series of experiments on the formation of alloys by precipitation out of aqueous solution. They find that when a plate of zinc, cadmium, or tin is introduced into a very dilute solution of a salt of a less positive metal, the dark flocculent precipitate which is formed on the surface of the plate contains not only the metal which was in solution, but also a considerable percentage of the more positive metal of which the plate is made. Thus zinc, introduced into a solution of copper sulphate, causes a precipitate which may contain as much as 50 per cent. of zinc, and which, when compressed and burnished, is evidently *brass*. Their explanation of the phenomenon is that at a spot where the copper has been electrolytically deposited and wholly removed from the liquid, it finds itself surrounded by a solution of zinc sulphate, so that, with the neighbouring zinc, a couple consisting of zinc, zinc sulphate, and copper is formed. Consequently a thin layer of zinc will be deposited on the copper. This would at once stop the action but for the diffusion of the zinc into the copper as an alloy. The surface of zinc being thus converted into one of alloy, more zinc will be deposited until the copper is saturated with zinc, that is, until the electromotive force of the couple zinc, zinc sulphate, alloy is zero. Laurie's work would lead us to expect that the electromotive force would vanish when the alloy had a definite atomic composition, and Mylius and Fromm found that this was sometimes, but not always, the case. The precipitates of zinc-silver, zinc-copper, and cadmium-silver do not appear to have a definite composition, but cadmium precipitates out of a 1 per cent. solution of copper sulphate a body whose composition corresponds pretty nearly to the formula Cu_2Cd. They found that cadmium in a dilute solution of gold trichloride forms an alloy which has exactly the composition $AuCd_3$, and they remark on the extreme readiness with which gold and cadmium combine. Perhaps the most interesting alloy formed by them is the body Cu_3Sn, which grows in a crystalline form on a plate of tin when immersed in dilute

[1] *Berichte d. d. chem. Gesellschaft*, xxvii., 630.

copper chloride or sulphate. This is Laurie's compound, and they draw attention to the fact that many observers have pointed out this alloy as possessing remarkable properties. The alloy Cu_3Sn has the greatest density of all copper-tin alloys, it does not liquate, and it is the best conductor of heat and electricity. It is difficult to deny its right to be regarded as a chemical compound.

Mylius and Fromm have made many experiments in addition to the above, and indeed have opened up a new field of research in which much fruit will no doubt be gathered.

Purely chemical methods have been employed very successfully by Deville and Debray in their study of the definite compounds formed by zinc or tin with some of the metals of the platinum group.[1] They found that palladium, platinum, rhodium, iridium, and ruthenium, if dissolved in a large excess of zinc, produce alloys that, treated with dilute hydrochloric acid, do not yield up the whole of the zinc. A considerable proportion remains apparently in combination with the noble metal. In the case of rhodium, iridium, and ruthenium there is a very large evolution of heat during their solution in zinc. The residual alloys after treatment with hydrochloric acid leave residues possessing a remarkable property. These residues, well washed and dried at 100° C., were heated in vacuo to 300° C., when there was a change of state accompanied by a large evolution of heat. We see here indications of chemical action even when alloys of definite composition have not been found.

Debray[2] has also studied the alloys produced by dissolving the above metals in an excess of tin, and in this process, as before, a large evolution of heat was observed. The ingot was afterwards slowly acted on by dilute hydrochloric acid. In the case of the platinum-tin, as the solution proceeded, brilliant plates were observed to stand out from the surface of the mass. These could be easily detached and analysed, and were found to have very exactly the formula $PtSn_4$. By a similar method crystals of the bodies $RhSn_3$

[1] *Comptes Rendus*, p. 1557, 1882.
[2] *Ibid.*, p. 1470, 1887.

and $RuSn_3$ were obtained. When osmium was dissolved in zinc or tin, and the alloy treated with hydrochloric acid, a residue of pure osmium was left, so that osmium appears to dissolve in these metals without combining with them. When lead or copper were used as solvents, similar evidence of chemical combination was obtained.

Heycock and Neville have found that when gold is dissolved in sodium, and the apparently homogeneous alloy is treated with alcohol, and finally with water, a beautifully crystalline mass is left consisting of lustrous microscopic needles. These are of a pale brassy gold colour, but when heated to about 200° C. they become incandescent and their colour changes to that of pure red gold. After ignition, but not before, the residual sodium can be extracted with water and estimated. The percentage of sodium agrees pretty well with the formula Au_3Na, but there is never quite enough sodium, probably because the solvent used to remove the excess of that metal attacks the crystals superficially.

By dissolving titanium in aluminium and extracting with hydrochloric acid, Levy[1] has obtained fine crystalline plates which appear to be a mixture of $TiAl_4$ and $TiSi_4$.

Joannis,[2] by treating the metals lead, bismuth or antimony with sodammonium or potassammonium, and removing the excess of alkali metal by washing with liquid ammonia, has obtained the bodies PbK, $BiNa_3$, $SbNa_3$, $PbNa_2$, analysis giving almost exactly the percentage corresponding to these formulæ.

These examples by no means exhaust the list of definite compounds that are known or suspected to exist. But although the evidence in favour of chemical combination is pretty conclusive in some of the above cases, it is well to remember that the original ingot of alloy may consist of crystals of the solvent metal imbedded in eutectic alloy, and that this latter, though not a chemical compound, may be such an intimate mixture of the two metals that reagents can only attack it superficially. We should then get a

[1] *Comptes Rendus*, p. 66, 1888.

[2] *Ibid.*, p. 585, 1892.

residue of constant composition which was not a chemical compound.

The microscope promises to become as important to the student of metallurgy as it has long been to the geologist, the connection between the mechanical properties of an alloy and its microscopic structure being a very close one. The work of Osmond and others on the microscopic structure of steel deals with a material too complex for adequate discussion here, and of the valuable treatise by Behrens on the minute structure of alloys, the same must be said. However the results obtained by the last-named writer, by Guillemin,[1] and by Charpy,[2] present certain general features. Guillemin etches the polished surface of a brass or bronze with dilute sulphuric acid; the etched surface is then examined microscopically, and the image photographed. Different alloys give very different surfaces, but the same alloy gives a surface of constant character. It presents more or less twisted furrows separated by parts standing out which the acid has spared. He says "it is certain that at the moment of solidification the metal undergoes a liquation, and separates into several alloys of definite composition, which are unequally attacked by the acid".

Charpy applies Guillemin's method, with modifications, to the study of brass, and concludes that the brass is formed of two alloys, one being crystalline and the other amorphous. When the alloy is heated the crystals begin to lose their shape at 700°, and at a rather higher temperature zinc volatilises from them. He utilises electrical action to bring about the etching, making the brass surface to be etched the zincode of a Daniell's cell. This method of effecting fractional solution appears a promising one.

Behrens in his work on *Das Mikroskopische Gefüge der Metalle und Legierungen* gives a vast amount of information on this subject. He etches either the natural surface or a polished section of the metal with a variety of solvents, and also brings out texture by partial oxidation produced by heat. The appearance of structure in the alloys he examined

[1] *Comptes Rendus*, 115, p. 232, 1892.
[2] *Ibid.*, 116, p. 1131, 1893.

is almost universal. A few alloys, such as Levol's eutectic copper-silver alloy, Cu_2Ag_3, and speculum bronze, $SnCu_3$, which Riche has shown to be free from a tendency to liquate, show no structure under the microscope; but all others can be made by etching, or surface oxidation, to show two constituents. This is very well seen in copper-silver alloys. In pure copper the boundary lines between the crystals can with difficulty be detected, but the addition of even a tenth of a per cent. of silver produces a network of slender white lines between the faces of the crystals of copper. These lines grow in width with increasing percentage of silver, and when the alloy attains the composition of the eutectic alloy, about Cu_2Ag_3, they form the whole surface, the crystals of copper having disappeared. With a larger percentage of silver, dendritic crystals of this metal can be seen, *so that in all mixtures of these two metals we find an alloy of mean composition filling up spaces between crystals of the metal present in excess.*

Exactly similar phenomena are shown by bronzes, and by many other alloys. It is hardly necessary to point out that these facts agree very well with the views expressed in the previous pages. Many of Behrens' results, however, suggest the conclusion that the proximate constituents of a solid alloy are frequently themselves alloys, and not the pure metals. As an example of the complexity of the phenomena observed by him, we give here his account of the microscopic structure of a sample of gun metal. "Ten per cent. bronze cast in bars. Well formed rectangular rosettes composed of cubes of 100μ in diameter. The threads of light yellow alloy rich in tin, and measuring 20μ, often have an almost white central line; moreover, most of the pale red cubes are surrounded by a narrow white edge, which stands out plainly from the pale yellow. Spots of tin were not present."

Steel, the most important of all alloys, will probably in the end be found to have a structure similar to the above described. Osmond finds nuclei of pure iron imbedded in cells of a substance rich in carbon and silicon, and Sorby, long ago, detected in many kinds of steel a microscopic

structure of plates of pure iron alternating with thinner plates of a carbide of iron. Osmond[1] considers, however, that as many as five crystalline ingredients can be detected in steel, and that iron itself can, by processes of hardening and tempering, be brought into at least three allotropic states. Let us hope that increasing knowledge will simplify and not further complicate the views held on this subject.

It is certain that although the scientific explanation of all the phenomena revealed by the microscopic examination of alloys may not come to us for some time, yet technical workers who use the microscope will find, and are finding, a correlation between microscopic structure and mechanical properties that cannot fail to be of enormous value to the engineer.[2] For example, the microscope throws light on that important section of metallurgy, the use of small admixtures of reducing agents, such as zinc or sodium with copper. It also promises to explain the curious change by which some bronzes are hardened by reheating.

Behrens, in common with many other students of the subject, is disposed to attribute definite chemical formulæ to some of the constituents found in an alloy, especially to those alloys which solidify homogeneously; but the writer inclines to the view that these eutectic alloys, such as Levol's Cu_2Ag_3, and others of the same kind, may be solid solutions and may have no claim to be regarded as chemical compounds. Whether this be so or not, it is certain that the arguments as to definite chemical composition brought forward by each worker from the use of his own methods, must be tested and supported by other methods before they can be generally accepted.

Dr. Foerster, in a valuable series of articles on the "Chemical Nature of Alloys,"[3] to which the present writer is indebted for several references, suggests that alloys of

[1] Méthode Générale pour l'Analyse Micrographique des Aciers au Carbone. *Bull. de la Société d'Encouragement*, x., p. 480, 1895.

[2] Roberts Austen, *Nature*, Aug. 15, 1895.

[3] *Naturwissenschaftliche Rundschau*, 1894.

definite composition, such as AuCd, Cu_3Sn and others, many of which do not obey the ordinary laws of valency, may be held together rather after the fashion of double salts, may in fact be *molecular* and not *atomic* compounds. In this case they may be dissociated when in solution, but like the alums, exist in the solid form. Hence in a dilute solution of copper in tin, the copper may exist, as Heycock and Neville have shown, in the form of monatomic molecules, but yet when the mass solidifies the bodies Cu_4Sn and Cu_3Sn may be formed. It is certainly somewhat difficult to imagine that the electropositive and self-saturating atoms of gold and cadmium can be held together, in the molecule AuCd, by forces similar to those which maintain the equilibrium of a molecule of hydrochloric acid.

Future progress in this subject will probably be the reward of those who bring physical as well as chemical and microscopical methods to bear on the same problem.

F. H. Neville.

ANTHROPOMETRY IN INDIA.

GREAT as are, and have been, the opportunities for anthropological research afforded by our Indian Empire, it can hardly be said that they have been utilised to anything like the extent that might have been the case. It is needless to dwell on the causes of this defect; they are numerous but sufficiently clear; and the parsimony of the Indian Government in matters relating to science is prominent among them.[1] But neither zeal nor industry have been awanting in European officials acting in their private capacity, and vast masses of ethnological material have been collected by them; the department of physical anthropology has, however, been comparatively neglected, and what has been done therein up to a recent date has not been done systematically. Thus, for example, Dr. John Shortt, on the inspiration of Dr. Barnard Davis, so long ago as 1868, published a long series of measurements of the several tribes of the Neilgherry Mountains, and of certain castes in the plain of the Carnatic; but he omitted to ascertain the length and breadth of the head; and his observations, though valuable, are much less so than they might have been.

Here, as generally elsewhere, the earlier attempts to affiliate and classify the races of the country were based on language. History gave very little assistance; in fact the Hindus can hardly be said to have had any history before the first onset of Mussulman conquest. But a rough division based on language was easily arrived at; the Hindus to the north of the Vindhya were recognised as members of the great Indo-European family, and the occupants of the Deccan, with their Dravidian tongues, were supposed to be the descendants of the primitive population, pushed down into the peninsula by the Aryan

[1] Mr. Risley pleads in excuse of the incompleteness of his work in the Punjab the fact that the government of that province was too poor to assist in it.

conquerors. What were the elements and affinities of that primitive population remained, however, very doubtful; and the problem grew more complicated and difficult with a more complete knowledge of the numerous hill, forest, outcast, or servile tribes found not only in Southern and Central India, but even in the far north and north-east. It seemed doubtful whether these could really be the survivals of the organised and powerful peoples who were spoken of in the old Aryan literature as the hostile occupants of Hindustan. A not unplausible idea, supported by the alleged presence of an outlying Dravidian language among the Brahuis of Biluchistan, was that the best parts of India had been conquered from the aborigines long before the Aryan invasion by a Mongoloid or Turanian people, of whom the Brahuis were the rear-guard. The aborigines might have been of some other type, perhaps akin to the Mincopies of Andaman, or of some other negroid stem. But some observers found Mongoloid features in them too. Dr. Shortt, certainly the most careful observer of the physical characteristics in the last generation, reported of the Kurumbas, a very degraded hill tribe in the Neilgherries, that they had "heads short from end to end, with a lofty crown or dome and prominent forehead, wedge-shaped faces, and obtuse facial angles, hollow cheeks, and prominent molar bones, the nose having a deep indentation at the root about $1\frac{3}{4}$ inches in depth (!), which is general". The length of the nose from root to tip he found to be 1·75 inches, the breadth 1·42 nearly; from which we may fairly conjecture that the Kurumbas are, if not platyrhine, at least very nearly so.

The value of these observations is impaired, for our present purpose, by the doubt whether the Kurumbas are not really a degenerate Dravidian tribe, once at a fair level of civilisation, rather than an aboriginal clan; but they are very interesting in relation to the Australioid hypothesis, of which more presently.

Again, with regard to the Yenadis, another outcast tribe of Madras, Shortt speaks of them as having a

Mongolian type of feature, with broad and prominent cheek-bones, a pointed chin, a scanty mustache, no whiskers, and a scanty straggling beard. Of nine skulls which he obtained, he says: "On a cursory view of these there is nothing remarkable among any of them; they are somewhat small and light, and whilst the female skulls are uniform in contour (*i.e.*, I presume, dolichous, and oval or elliptic in the norma verticalis), the males have a great parietal width". Two of the female crania, which he presented to Barnard Davis,[1] are now in the museum of the College of Surgeons; they are small and dolichous (cranial index, 70·9), and the zygomata are by no means wide. In fact it was clearly on the facial aspect and the scanty development of hair that Shortt founded any ideas he may have had of the Mongolian affinities of these outcasts, and these he was unable to put to the test of measurement, which, indeed, at that date had hardly begun to be applied to the face by anthropologists. Width of the cheek-bones is not necessarily accompanied by width of the zygomata, and is, of course, characteristic of other races besides those which we style Mongoloid or Turanian. It is very general among negros and negroids; and in relation to the breadth of the head even that of the zygoma is almost as often great in the black as in the yellow races.

But Huxley soon brought the search-light of his keen observation to bear upon the people of Southern India, with the result that he detected the resemblance existing between them and the Australians, with whose type he was familiar. The small long head, the deep nasal notch, the broad nose, the dark skin, the black wavy hair were characters which warranted him in denominating the Dravidians Australioids.

Strangely enough, very little has been done in Southern India since Shortt's time, in the way of extra observations; but in East Central India the Kolarian tribes have been a good deal studied. The linguistic separation between Kolarians and Dravidians was sup-

[1] *Thesaurus Craniorum*, vol. i., p. 157.

posed to be accompanied by a difference of physical type; and the former were thought to be the descendants of Mongoloid immigrants from the north-east, the latter to be either the remains of an ancient oceanic population, or to have migrated from the north-west from the region where the Brahui had been left behind.

New light has been thrown on the subject, however, during the last few years by the investigations made by Mr. H. H. Risley, with governmental authority and support, into the anthropometry of the Bengal presidency.

The ethnology of this part of India—the valleys, roughly speaking, of the Ganges and the middle Indus—had never appeared so difficult as that of the peninsula, though it had its own complexities, and the absence of historical data was almost as deplorable. The prevailing view was that the primitive Aryans, entering the plain of the Indus from the north-west, had gradually spread eastwards, conquering and civilising until they had occupied almost the entire tract. Some, those who made light of colour and complexion as permanent racial characteristics, supposed that the whole population was of Aryan descent more or less pure, and this view was naturally not unwelcome to the educated and patriotic Hindu. Others believed that the twice-born castes, the Brahmans, Kshatriyas and Vaisyas, were truly Aryan, the Sudras, or mass of the population, being descended from the conquered Dasyus, the ancient occupants, more or less leavened, perhaps, by a tincture of the blood of the superior race. It was natural, also, to suppose that, in accordance with the usual rules of migration and conquest, the proportion of the blood of the invaders would be found greatest in the neighbourhood of the scene of their arrival, on the north-western frontier, that is, and in the Punjab; though the distribution would no doubt be modified to some extent by the greater or less attractions of the soil in the several provinces; thus aboriginal tribes were known to have subsisted in force till a late period in the arid hills and plains of Rajputana, and in the forest tracts of Central India, whereas the Doab of the

Ganges and Jumna had since the dawn of history, and doubtless long before, been the aim and coveted possession of every invading tribe, Iranian or Turanian, Greek, Pathan, Turk or Mongol.

Different theories of caste complicated the subject. Caste means colour, and the prevailing opinion was that of the Hindus themselves, that the origin of castes was in the distinctions of races who differed physically and palpably. Their great multiplicity was ascribed partly to breach of laws of endogamy by cross-breeding. Thus the Ambastha, or physician caste, was supposed to descend from the intermarriage of Brahmans with Vaisyas. On this theory, however, there is a little difficulty in perceiving why a particular cross-breed should have been confined to a particular occupation, and there were those who preferred to assign the origin of caste distinctions to difference of occupation rather than of blood, and who thought the prohibition of exogamy was of later date. There are, indeed, indications that these prohibitions were not so absolute or so strictly adhered to at some remote period as they have been now for many ages; and it will be found that this will furnish an almost necessary explanation of certain general resemblances in the provincial populations.

Mr. Risley, taking his method and instructions from Topinard and Flower, and using chiefly native assistants, who appear to have carried out their work with great intelligence and zeal, ascertained the stature and weight, and the principal kephalic and facial measurements in a considerable number of individuals of each of 87 castes or tribes, of which 15 belonged to the province of Bengal, 5 to Chittagong, 10 to the Darjiling Hills, 10 to Behar, 17 to Chota Nagpur, 23 to Oudh and the north-west provinces, and 9 to the Punjab. In a good many cases he succeeded in getting 100 subjects of each caste, but most of those in the Punjab, and some in the other provinces, are but insufficiently represented. The total number of persons examined was nearly 6000.

The chief inferences which Mr. Risley drew from this

extensive investigation were, first, that the castes are, generally speaking, really ethnological and not merely social divisions; second, that three main types may be distinguished in the population of India at the present day.

1. A leptorhine (long-nosed), pro-opic (*i.e.*, hatchet-faced, or as the Chinese say of us, horse-faced), dolichokephalic type, of tall stature, light build, narrow face, comparatively fair complexion, and high or open facial angle. This type is most marked in the Punjab. Their exogamous groups are eponymous, names of Vedic saints or heroes.

2. A platyrhine, mesopic or nearly platyopic, dolichokephalic type, of low stature, thick-set, with very dark complexion, broad face, and usually low facial angle; most distinct in Chota Nagpur and the central provinces. Its sections are totemistic, names of animals, plants, etc., to all of which some form of taboo applies.

3. A mesorhine, platyopic, brachykephalic type, of lower medium stature, sturdy build, yellowish complexion, broad face, and low facial angle; found along the northern and eastern frontiers of Bengal. Exogamous groups curiously named, mostly by nicknames of supposed founders.

Of these the first is evidently the Aryan type, the type, that is, which has from time to time filtered in from the north-west across the Indus. Except in colour, it recalls the tall blonde type of Northern Europe; except in stature, perhaps, as Topinard suggests, the dark Mediterranean type. The dolichokephaly is probably inherent in the immigrant stock, not acquired by admixture with the black Australioids, for it is as strong among the Punjabis (abstraction having been made of the trans-Sindhian Pathans and Biluchs) as anywhere, and would not, in the Sikhs and Gujars, exceed 71 in the bare skull. It exists, moreover, in an exquisite degree among the north-western mountains, in the Cashmiris, the Dards, and Chitralis, people who, from their position in the direction of the supposed fountains of migration, should be more Aryan than the most

Aryan Hindus, should be in fact to them what the Holsteiner or Frisian is to the Englishman.

These Dards, be it noted, though a few among them may be blonde, seem to be generally dark-eyed and dark-haired; and the same seems to be the case with the Kafirs of the Hindu Kush.

Topinard in his commentary on Risley's tables hints his surprise at the smallness of the influence which the Turanian conquerors (Turks, Mongols, perhaps Saka) have had in raising the kephalic index. It seems to me that these invaders, in the historical period at least, usually crossed the Punjab to settle in the richer valley of the Ganges. It is true that there is no indication of them in Risley's figures for the north-west provinces, either; but then he has not included the up-country Mussulmans, Moguls, Rohillas, etc., in his observations, which seems to be an unfortunate omission.

Another thing which some may regret is the exclusion from the scheme of any observations on colour. Mr. Risley appears to have been unacquainted with Broca's colour-scale (which, though not applicable to the hair, is fairly good for the skin), and to have thought that no easily workable system of colour-observation existed.

The Punjabis are, as is generally known, strikingly superior in stature to the people of other parts of India. Of 80 Sikhs measured the range was from 165 to 190 centimetres (65 to 74·8 inches), and the average 171·6 (67·5 inches); and the lowest average for a single caste in the Punjab (165·8 for the Aroras) surpasses the highest for any caste in Bengal (165·6 for the Brahmans). In weight, however, they fall below the burly mountaineers of Sikkim; and even among the stunted hillmen of Chota Nagpur there is one tribe, the Munda, which excels them. The proportion of weight to height varies, therefore, very remarkably, from 372 in the Mundas to 290 in the Khatris of Oudh, a caste supposed to have a strong Aryan element, the average for the whole of Bengal being apparently 308. On the whole the tall slender frame seems to characterise the partly-Aryan Hindu to-day, as it did in the Vedic period, when his

supposed ancestors derided the stumpy, noseless, black aborigines; still, it may be that differences in elevation of habitat have exerted some influence.

Other and more unequivocal characteristics seem to be those derivable from the breadth of the nose, and somewhat less positively from the naso-malar index of Flower and Oldfield-Thomas, on both of which great stress is laid by Risley, whose figures seem to bear out his opinion.

The average nasal index of the Punjabis comes out at 70·2, just on the limit of the leptorhine and mesorhine class, but the Sikhs, Pathans, and Biluchs are actually leptorhine, and the index for the Goojurs, the very interesting pastoral caste of the Punjab, is as low as 66·9; unfortunately only 13 Goojurs could be examined. These figures are perfectly European.[1]

The nasal index does not, as might perhaps have been expected, increase with anything like regular progression as we advance eastwards from the Punjab; but it does increase, and that considerably. Something should perhaps be allowed for the fact that a partly arbitrary, partly fortuitous selection of castes cannot be relied on to give the absolute means of the whole population; and something, certainly, for the personal equation of the observers; for the exact measurement of the nose is one of the most difficult bits of anthropometry. But it seems strange that Risley's Bengalis yield a nasal index of 78·7, his Bēharis one of 80, and his men of the upper Ganges (Oudh, etc.) one of 80·9. Similarly, his Brahmans of Bengal give us 70·4, those of Bēhar 73·2, those of the upper Ganges 74·6. Can it be that there is actually a larger Aryan element in Bengal than in the country between there and the Sutlej? That seems very improbable. On the other hand, the Mongoloid brachykephalic people of the Darjiling Hills have a comparatively low mesorhine index—only 74·7 on an average, and the Lepchas of Sikkim are positively leptorhine, while the people of the Chittagong Hills have a mesorhine index of 82·7, not nearly so high as that of the more dolichous

[1] I found the nasal index 69·1 in 50 Englishmen, mostly of the upper class.

hillmen of Chota Nagpur (87·4). The low-caste men of Bengal proper have a less prominent nose root (which is a simpler way of saying a smaller naso-malar index, 111·7 to 113·8) than those of Bēhar; though more prominent than the people of the Chittagong Hills, of Sikkim, and the Himalaya, or than them of Chota Nagpur, which facts, on the other hand, would seem to imply a greater infusion of Aryan blood in Bēhar than in Bengal. The Chota Nagpur people again are not quite so flat faced as—have a rather more prominent nose root than—the Chittagongers and Sikkimites (110·4 to 107·1 and 108·6). And lastly, the Bengalis approximate somewhat to the last-mentioned tribes in kephalic index, the order in that respect being as follows:—

Oudh, etc.,	72·8[1]	Bengal,	76·9
Punjab,	74·2	Chittagong,	79·9
Chota Nagpur,	74·9	Sikkim, etc.,	80·7
Bēhar,	75·7		

and the high average of the Bengalis not being due to the low-caste men included; for it affects both high and low, the very highest averages being those of the Brahmans (78·7), the Kayasths (the writer caste, of doubtful origin but high in rank) (78·3), and the Chandals, a very low caste (78·1). The average of the Brahmans of Bēhar is only 74·9, and that of those of Oudh and the north-west 73·1 apparently.[2]

Here seems to be a kind of crux. The most likely ways out of it are the following, for some of the discrepancies seem beyond the possible range of the personal equation.[3]

[1] See further on.

[2] This is possibly an under-statement; see further on.

[3] Mr. Risley's figures, having so wide a basis, need no confirmation except with reference to the possible interference of the personal equation. I may mention, however, that five Bengali Brahmans and Kayasths, whom I measured long ago, yielded an average index of 80·3. The measurements in Barnard Davis's *Thesaurus*, a great many of which refer to the crania of natives of Dacca in Eastern Bengal, are also corroborative. Davis mentions a very important point, that Bengali skulls are often obliquely deformed, probably from some peculiarity in the way of nursing: this may perhaps a little exalt the index.

Either (1) the case must be put down to the credit of the transformists, and we must suppose that the influence of media in Bengal tends towards brachykephaly. The only influence of the kind I can suggest is that of the drinking water, which in that great alluvial plain may be deficient in calcareous salts.

Or (2) we must suppose that the views of Col. Dalton and his followers are so far correct that there was at some time or times anterior to history a considerable immigration into Eastern India from the north and east, consisting of course of brachykephals, who would permanently modify the cranial form of the inhabitants of Bengal proper, but who did not push forward into Orissa and Chota Nagpur in sufficient numbers or force to produce any permanent effect on the prevailing type there.

Not that this hypothesis (which, by the way, seems to be favoured by Mr. Risley)[1] is not free from very strong objections. Thus, it is generally supposed, and claimed by themselves, that the Bengali Brahmans have been strictly endogamous at least since their arrival in their present seats, whither indeed the immigration of some of their subsections is asserted to have been comparatively recent. We can but suppose that during the period when Buddhism had sway in Bengal its doctrine of equality prevailed to break up for the time the Brahman law of endogamy, which was re-established and made strict, and society reorganised, on the downfall of that faith. Thus only, too, can we account for a degree of family likeness in most of the Bengali castes, the despised Chandals, for example, coming out with dimensions not much unlike the lordly Brahmans.

We pass next to the second type, which may be shortly named Negroid. Mr. Risley would call it Dravidian, but this seems hardly justifiable as yet, until we know through more numerous and exact observations in other parts of the great region which is Dravidian in language, that this is

[1] Marco Polo gives an interesting account of the victory of Kublai Khan over the King of Mien (Burmah) and Bangala, about 1280 A.D.; but we cannot be confident that the Burmese were really the lords of Bengal at that period.

really the type generally distributed through it. As yet we can only say that certain low or outcast tribes in the south seem to resemble those of Chota Nagpur, whether Kolarian or Dravidian in language, which have been examined. Ceylon is the only portion of the great south where a Sanskritic tongue prevails, and though it is always dangerous to argue from language to race, it is tolerably safe to say that if at any one time and in any one province of the south an Aryan colony had settled in numbers anything like equal to those of the natives, they would have succeeded in importing and transmitting their language. It is on the whole, then, improbable that anywhere in the peninsula Aryan blood predominates; yet we are told that in many parts the prevailing type is handsome and neither Mongoloid nor Negroid or Australioid in aspect.[1] It is best to suspend our judgment as to the component elements of these southern people.

The nasal index is the most characteristic point in the measurements of this type. In the valley of the Ganges, except in Bengal proper, it seems to be the invariable rule that a high caste is distinguished from a low one by the smaller breadth of the nose. Of course this is true only *en masse*, and not always in the individual; even in the Sikhs, reputed (I believe) an extremely homogeneous tribe, this index varies from 55 to 82. Of the naso-malar index I am not disposed to think so highly, at least in its application to the living. The Chandals in Bengal, and the Doms in Chota Nagpur, come out by that test better than the Khatri of the Punjab. It must also be very difficult in practice. But, as Topinard says, it is well worthy of further trial.

The third type is the one usually recognised as Mongoloid or Turanian, and is prevalent in two of Risley's districts, the Darjiling Hills (*i.e.*, Sikkim and its neighbourhood), and the Chittagong Hills to the east of Bengal. The northern people are taller and considerably heavier

[1] The Todas of the Neilgherries, for example, who are tall, handsome in feature, and evidently not platyrhine, yet with but little except their physical aspect to dissociate them from other Dravidian-speaking tribes.

than the eastern, but in other respects no great differences appear in Risley's tables, except that his frontozygomatic index (the proportion borne by the minimum breadth of the forehead to the maximum of the zygomata taken as 100) is larger in Sikkim than in Chittagong as 76 to 74. Topinard, in his masterly review of Risley's work, objects to this index as of little value, because the minimum frontal breadth gives little or no indication of the breadth of the frontal lobe. The really valuable index, as he shows, is got by comparing the stephanic or maximum frontal breadth with the zygomatic. But in the living subject this is extremely difficult; and though I myself employ Topinard's index with profit, I am conscious that my data are often wide of the fact. On the other hand the fronto-minimal plan is easy though untrustworthy, and on the whole and in the main, or in a certain number of cases, does point in the desired direction. Thus the index so gotten singles out the Brahmans from the other Bengalis (82·4, the next in order being the respectable caste of Kayasths with 79·3), and thus brings them into line with the nine Punjabi castes or tribes, who with the exception of the Kachis in the N.W. Province are the only ones which rise above 80. Both sections of Mongoloids have faces longer as well as broader than Hindus, whence the vertico-kephalic index, or proportion of the maximum breadth to the total height of the head (chin to vertex), is not very significant.[1] And I cannot help entertaining serious doubts of the entire trustworthiness of the figures for Oudh and the N.W. Province, in which I seem to detect a marked personal equation,

	[1] Length of Head.	Breadth of Head.	Height of Head and Face.	Height of Forehead (vertex to inter-superciliary point).	Ear Height.
Punjab - - - -	187	139	218	86	135
Sikkim - - - -	184	149	220	82	135
Chittagong - - -	182	146	218	83	135
Bengal - - - -	183	141	208	76	129
Chota Nagpur - -	185	139	208	79	127
Bēhar - - - - -	185·4	141	208	78	129
Oudh & N.W. Prov.	185	135	222	99	127

operating in the direction of an exaggeration of the vertical height and probably also a diminution of the breadth of the head. Thus, if we compare, in the adjoining provinces of the north-west and of Bēhar, castes identical in name, and, we may presume, nearly identical in blood, we get the following results :—

		Length	Breadth	Vert. Height	Do., Forehead	Ear Height
Brahman -	Bēhar - - -	187·8	140·8	214	83·7	132
	Oudh and N.W. -	187·5	137·2	223	103·3	130·4
Babhan -	Bēhar - - -	187·8	144·4	214	80·7	132
	Oudh and N.W. -	187·2	137·4	222	99	129·5
Goalà -	Bēhar - - -	185·4	141·4	209	78	130
	Oudh and N.W. -	185·2	135·2	217	96·7	127
Chamar -	Bēhar - - -	184·4	140·3	205	76	129
	Oudh and N.W. -	185·1	134·9	221	99	127
Musahar -	Bēhar - - -	183	138·6	202	73	127
	Oudh and N.W. -	181·7	134·8	214	92	130
Mean of 5	Bēhar - - -	185·7	141·1	209	78·3	130
	Oudh and N.W. -	185·3	135·9	219	98	128·8

Here the correspondence of the figures in the first and fifth columns is so close that the wide discrepancies in the second, third, and fourth columns become almost incredible ; and as they invariably run in the same directions of less breadth and greater height, for the castes of Oudh, etc., one is almost irresistibly led to attribute them, at least in part, to the personal equation of the observer. It is not difficult to conceive the habitual error in the *horizontation* of the head, which has led to the errors in the third and fourth columns : in the second the fault, if fault there was, lay probably in measuring at the spots where the maximum breadth might be expected to be, instead of groping about to find the maximum.[1] I see no internal evidence to lead one to distrust the other measurements, but of course all the indices for Oudh and the N.W. Province, into which the vertical height enters as an element, must have come out too small.

The very extensive distribution of some of the castes supplies an argument against the ethnological explanation

[1] Or it may be that the Oudh operator put a little more pressure on the callipers than did his fellow-workers. The frontal and bizygomatic measurements for Oudh, etc., are smaller than those for Bēhar, but not beyond the limits of likelihood.

of caste. Thus the Doms are found by name not only in the Gangetic provinces, in Chota Nagpur, and (I believe) in Orissa, but also in the far south, about Madras. And their occupations in these different localities seem sufficiently alike to lead one to identify them as, at least, the same caste everywhere. Are we to suppose that their wide spread is due to migration? In the case of the Brahmans and of the military castes or races, one would have no difficulty in believing this; but does it not seem difficult in the case of Helots like the Doms?

The study of the nasal index is enough by itself to make me believe in the racial origin of the system; but it seems quite possible that castes were formed subsequently by the comradeship of people of like occupation. In recent times and in our own day, castes would have been and are being formed out of aboriginal tribes low in the scale of civilisation or social status; and of this Mr. Risley gives many interesting and sometimes amusing instances. The chiefs of such a tribe almost invariably endeavour to hang themselves on to some branch of the Rajputs; while the proletariat constitute a new caste, for which an Aryan ancestry is sometimes claimed. Outcasts from higher castes are sometimes received into lower ones; and this may be one of the ways in which a circulation of blood has been set up, so as slightly to assimilate the various sections of the population.

The small average size of Hindu heads is remarkable. It comes out very distinctly in Barnard Davis's *Thesaurus*, as does their inferiority in this respect to those of the Mongoloid races to the north and east of their habitat. Even the Punjabi heads are not large, nor apparently those of the dolichous tribes north-west of the Indus, Dards, Kafirs, etc., of whom Leitner, Garson, and others have written. Admixture of Dravidian or Australioid types could only be expected to lower the cranial capacity; admixture of Mongoloid ones might possibly counterbalance this. Mr. Risley's facts seem to follow these rules. I have constructed a table of comparative sizes by multiplying in every case the maximum length by the maximum breadth,

and that by the ear height. A trifle less than two-fifths of the product would probably give the cranial capacity in cubic centimetres.[1]

Relative size of head in the several provinces :—[2]

Brahmans, Bengal	-	-	3474
Kayasths, ,,	-	-	3394
Province, ,,	-	-	3347
Bagdis, ,,	-	-	3277
Babhans, Bēhar	-	-	3653
Brahmans, ,,	-	-	3496
Province, ,,	-	-	3384
Musahars, ,,	-	-	3218
Brahmans, N.W. and Oudh			3347
Chhetris, ,,			3342
Babhans, ,,			3330
Kayasths, ,,			3239
Province, ,,			3197
Kols, ,,			3083
Pathans, Punjab	-	-	3626
Goojurs, ,,	-	-	3602
Province, ,,	-	-	3521
Chuhras, ,,	-	-	3393
Cheros (Ch. Nagpur)	-	-	3544
Province ,,	-	-	3346
Bhuiyas ,,	-	-	3229
Sikkim Province	-	-	3726
Chittagong Province	-	-	3585

Here the brachykephals surpass the dolichos, the Aryans the aborigines, the upper surpass the lower castes. But the Brahmans, despite their claims to be considered a kind of intellectual aristocracy, do not seem to surpass the other high-caste men, unless it be in the dimensions of the forehead ; and the Kayasths or writers, almost all of whom live by their brains and their pens, do not stand very much above the average. These differences are not inconsiderable ; they exceed probably those which obtain between the superior and inferior classes in our own country ; but I do not think they yield any evidence in support of the inheritance of acquired characters.

[1] Thus : high-caste men (chiefly Bengali), 72 cubic inches capacity ; low-caste men, 71 ; Central India, 64·5 ; Lepchas (Sikkim), 73·8 ; Bodos, 75·5 ; Bodpas, 76·5 ; Mishmis, 77 ; Thai (Siamese), 77·6. (B. Davis.)

[2] The Chhetri (Rajputs), Babhans, Goojurs, have strong claims to be considered largely Aryan. The Kayasths, or writer caste, though now high in rank, are probably much less so. The Cheros were anciently a royal race. The Bagdis, Bhuiyas, Kols and Musahars (hunters) are distinctly aboriginal. The Chuhras are a helot caste.

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JOHN BEDDOE.

THE PRESENT POSITION OF FLORAL BIOLOGY.

DURING the last twelve months several important books and papers (1) dealing with the natural history of the flower have been published, and it may be well at this period to review our position, and also to endeavour to trace the general ideas that hold sway in this department of knowledge. The republication of C. K. Sprengel's *Entdeckte Geheimniss der Natur* (1793) enables any one easily to compare our present knowledge with his, and it is rather humiliating to find how very little we have advanced in the direction of deeper understanding of floral phenomena. We have indeed accumulated a stupendous mass of details, but except that for Sprengel's old-fashioned teleology we have substituted the modern one based upon evolution, invoking "adaptation" where he refers to "design of nature," we are really not much further forward. The chief advances made have been effected by Darwin and by H. Müller, who have given us the "Knight-Darwin-Müller hypothesis" and the "Theory of Flowers". No branch of biology has suffered more than that under consideration, from the pushing to extremes of the adaptation idea. The dangers of this course have often been pointed out, and in other branches not without good effect; but a perusal of almost any work upon floral biology will show that here at least, adaptation is made to explain almost everything; biological meanings are forced into every detail of floral structure, often without any evidence whatever. The fact that the subject is so admirably suited for popular treatment, is probably responsible for much of this.

Rather than confess that we do not know the meaning of a particular structure or phenomenon, we give a glib "explanation" by saying that it is an adaptation to something. So far has this gone, that even among professional botanists there is an idea, perhaps unconscious, that the subject is exhausted and affords no room for good research,

whereas, in reality, no branch of vegetable physiology offers more, or more interesting, problems for solution. One factor that has deterred many from work of this kind has doubtless been its laborious nature; few persons are willing to undertake experiments or observations lasting over several years, preferring to do something which may be speedily finished and put into print.

With this introduction, we may go on to consider the chief departments of the subject. Of the various treatises above-mentioned that deal with the whole subject, that of MacLeod is much the best for treatment of principles, while Loew's works give all possible details. Müller's book, though a classic, is now very seriously out of date, and is no longer suited for general readers or for students. Much of what follows is based upon MacLeod's work; it is unfortunate that the book itself, being in Dutch, is available only to a limited circle of readers (it has a French abstract). Starting with the more general ideas that underlie the whole subject, we shall deal first with

CROSS-FERTILISATION AND THE KNIGHT-DARWIN HYPOTHESIS.

As stated by Darwin, this is "that no organic being fertilises itself for a perpetuity of generations, but that a cross with another individual is occasionally—perhaps at very long intervals—indispensable". By his work on orchids, Darwin rehabilitated Sprengel, and gave a fresh impetus to the study of floral mechanisms. In the writings of the earlier workers (*e.g.*, in Hildebrand's *Geschlechtervertheilung*, 1867) may be clearly seen the tendency to explain all flowers as adapted in greater or less degree to cross-fertilisation, whilst self-fertilisation is usually regarded as being actually harmful. This mode of looking at the subject is natural enough when one takes as a starting-point such a group of plants as the orchids, with their extremely complex mechanisms for obtaining cross-fertilisation. Darwin's experiments upon cross and self-fertilisation show that the latter is not actually harmful in itself, but that, *in most cases, its offspring will*, ceteris

paribus, *be inferior to the offspring of cross-fertilisation, if placed in direct competition with them.*

At the same time that these researches were going on, many cases of regular self-fertilisation were being discovered, though in all of them, as the flowers were open and often contained honey, there was a possibility of a cross taking place. Müller pointed this out, and proposed to substitute for the Knight-Darwin hypothesis, another, based upon the above facts, *viz.*, "that cross-fertilisation results in offspring which vanquish the offspring of self-fertilisation in the struggle for existence". Cross-fertilisation was still regarded as the chief object, so to speak, of a flower, self-fertilisation as a *pis aller.*

Until recently no cases were known of plants which always fertilise themselves and had no chance of a cross. Burck has shown that *Myrmecodia* and other plants never bear open flowers, but always fertilise themselves in cleistogamic flowers.[1] Hence the Knight-Darwin hypothesis must be abandoned in favour of some more general hypothesis, which will include these cases. Naegeli, as a result of his experiments upon hybridisation, laid down the law that "the consequences of fertilisation reach their optimum when a certain mean difference in the origin of the sexual cells is attained". The experiments of Darwin and others support this law, though they are contradictory when applied to the Knight-Darwin hypothesis. The latter is seen to be a corollary, for certain cases, of Naegeli's law. We may, therefore, for the present, adopt this law as a basis for further study; MacLeod proposes further hypotheses wider than that of Müller, based upon this law, and upon Darwin's experiments.

That cross-fertilisation, in itself, is advantageous, is probable for most cases, but there are at least three factors in the resulting gain, which ought to be considered separately and not lumped together as is usually done. These are (*a*) fertility of the parent plant, (*b*) strength of constitution of the offspring, (*c*) fertility of the offspring. Darwin measured

[1] Cleistogamic flowers = closed self-fertilising flowers.

all these and showed that in most cases there was a gain in all three, but that it was usually very different in each ; a large increase in one is generally accompanied by a small increase, or even a decrease, in one or both of the others. If we represent the value of a in crossed plants by a_x, in self-fertilised plants by a_s, we find, for example, in *Dianthus caryophyllus*—

$$\frac{a_x}{a_s} = \frac{100}{92}, \quad \frac{b_x}{b} = \frac{100}{86}, \quad \frac{c_x}{c_s} = \frac{100}{65},$$

whilst on the other hand, in *Nicotiana Tabacum* the ratios are $\frac{100}{150}$, $\frac{100}{178}$, $\frac{100}{97}$ (second generation in each case). So that, in considering the advantage (if any) of cross-fertilisation in any given case, each factor must be treated separately, remembering at the same time that a certain (numerical) gain in a may mean more to a certain plant than an equal gain in b or c, and so on. We must remember further that *the plant has to pay a certain price for its cross-fertilisation, viz.*, the material and energy represented by large corollas, honey, scent, extra pollen, big stigmas (in wind-fertilised flowers), etc. The price to be paid will vary considerably, even in the same species, being affected by the climate, the competition of other flowers, the number and nature of the insects in the locality (these varying from place to place, and from month to month of the year), and other factors. Again, cross-fertilisation means delayed fertilisation, and in the short summer of arctic or alpine climates where there is but little time for the ripening of seed, this may become an important point.

Similarly there will be variation in the gain to be derived from cross-fertilisation in any given case. The factors a, b, c, will enter into it in different degrees and sometimes one may be more advantageous than another. We know too that the fertility of a plant varies from one country to another, so that the gain derived will vary too. A further advantage probably lies, to many plants, in the increased variability resulting from cross-fertilisation. It is thus evident that in dealing with the natural history of a flower we have a very complex problem to solve, and must

make up for every plant in any locality a sort of balance-sheet of profit and loss before we can fully understand its floral mechanism. *Only when the gain derived from cross-fertilisation is greater than the price to be paid for it, will it prove advantageous and be retained by the plant.* It is important to remember, in this connection, that in all probability the flowering plants began with cross-fertilisation, handed down to them by their cryptogamic ancestors.

Upon this enlarged basis we can now understand better the great variation in floral mechanism exhibited by the same species in different parts of its distribution area, and it becomes of interest to study it in every possible locality. We might suppose such a case as this: Two plants A and B of different species are removed from a district rich in insects, where they were both crossed, to a district poor in insects. If crossing had proved of great advantage to A, but of small to B, then we may expect to find that A will acquire increased attractiveness, paying the extra price demanded for cross-fertilisation, whilst B will tend towards self-fertilisation, the extra price being too great to make crossing any longer an advantage, whilst by reduction of corolla, etc., a distinct saving is made, which may be devoted to extra storage in the seeds, vegetative reproduction, or other purposes. It will be asked, why do not such flowers as B become cleistogamic, and so make the utmost possible saving? On the older view, it was said that they remained open upon the chance of obtaining a cross, but this no longer suffices as an explanation. It seems possible that the opening of the flower, the honey-secretion, etc., may be useless survivals from insect-fertilised ancestors, and that the necessary circumstances (see below) have not arisen which would cause their complete reduction.

The world-wide distribution of many self-fertilised plants has always been difficult to explain, but Wallace has proposed a solution which harmonises with Naegeli's law, suggesting that small changes in conditions of life, such as are obtained by moving into new localities, may yield the same advantages as crossing of different stocks in increased fertility and flexibility of constitution. The wide distribu-

tion of highly specialised cross-fertilising flowers is largely prevented by the lack, in new districts, of insects suited to them. Cross-fertilised wind flowers, on the other hand, are often widely scattered, and so are the plants of many orders such as Compositæ, whose mechanism is very effective for cross-fertilisation, but not specialised for too narrow a group of insects. To say, as many writers from Axell onwards have said, that self-fertilised flowers are the highest type, and best fitted to survive in the struggle for existence, is to go as much too far towards one extreme as the earlier workers went towards the other.

MacLeod makes a tentative grouping of flowering plants into two classes—"capitalists" and "proletarians". The first group is composed of plants which have large reserve stores and can afford to pay for cross-fertilisation by producing large and attractive flowers, etc., while still retaining enough material to supply the seeds. Such are our trees and shrubs, most perennial herbs and many annuals, whilst the second group is composed chiefly of annual and ephemeral plants whose supply of material is all or nearly all used for seeds, and which therefore, being unable to pay for crossing, are self-fertilised. Such are *Alchemilla arvensis*, *Scleranthus annuus*, *Radiola linoides*. The idea is suggestive and brings out a point to which too little attention has been paid (though Darwin saw its importance), *viz.*, the great advantage to a plant of having its seeds provided with a large supply of reserves, thus enabling them to germinate rapidly and get the start in the struggle for light and air. The few data (2) we possess upon the advantages of heavy seeds as against light ones go to show that the advantages of the former are at least as great as the advantages of crossing, so that it might well pay a plant to fertilise itself and produce heavier seeds rather than try to obtain cross-fertilisation. Of course there is here also a balance to be struck between conflicting elements. The heavier the seed the less its chances of distribution. Again, in the case of annuals, where the seeds are liable to germinate in autumn and be killed by the frost, it becomes advantageous to produce great numbers of seeds

rather than heavy seeds, and so on. Plants with a good distribution mechanism can, on Wallace's hypothesis, better afford to do without cross-fertilisation. When both occur together, we get cases like the Compositæ, of widely scattered dominant and aggressive families.

Another great factor in a plant's life history is vegetative reproduction. Similar considerations apply to this as to the two great factors already considered. In itself the most certain and economical mode of propagation, it has to be paid for by the loss of the advantages of fertilisation and rapid distribution, and so we do not find many plants possessing only this means of multiplication.

FORMS OF FLOWERS.

Since the publication of Darwin's work but little has been done until quite recently (3). Vöchting has experimented upon the effects of light upon the production of cleistogamic flowers. In species which in nature produce both these and ordinary flowers, a reduction of light to a sufficient extent causes the production of cleistogamic flowers only, and in all cases reduction of light tends to hinder, or even prevent altogether, the full development of the flower. Other workers have shown that probably low temperature, poor soil, etc., may also act as factors in the production of cleistogamy. It would thus appear that cleistogamy is a phenomenon largely dependent upon external causes, although in many plants it is sufficiently fixed and well-marked to be termed an adaptation.

Vöchting's experiments also bring out the interesting point that reduced light causes increased vegetative multiplication, which is in accord with the actual facts observed in northern regions, etc.

Gynodiœcism, with its allied phenomena, has also received considerable attention. A species is said to be gynodiœcious when there occur two separate forms, one bearing female the other hermaphrodite flowers. If both kinds of flowers appear on one plant we speak of gynomonœcism. If one form bear male, one hermaphrodite flowers, the species is androdiœcious. The great number of

observations made upon the floras of different regions enable comparisons to be instituted, and it is found that a species may be gynodiœcious in one country and not in another; and that, further, in any one place, the proportion of hermaphrodite to female flowers may vary enormously, both from one year to the next, and within the limits of a single year. A plant which is at one time hermaphrodite may even change to female in a short time, or *vice versâ*. The phenomenon may under certain circumstances (*e.g.*, in early spring or in late autumn) appear in almost any plant. Again, nearly all species which are usually gynodiœcious exhibit gynomonœcism also, female flowers occurring in greater or less number upon the hermaphrodite plants. Gynomonœcism also is a phenomenon varying in degree and from place to place.

So variable and inconstant is gynodiœcism that it can scarcely be regarded as hereditary, though it usually appears in certain Labiatæ and one or two other plants. It has not been shown to be in any way advantageous to the species possessing it, but it has been taken for granted that it is an adaptation to cross-fertilisation. After what has been said above it would seem better to remove it altogether, at present, from the realm of adaptation, and study it more thoroughly on its own account.

Similar considerations apply to the phenomena of andromonœcism, etc. Here the male flowers are usually at the distal end of the inflorescence or on small lateral twigs, and their production almost certainly depends on lack of nourishment. [Somewhat similar phenomena are seen in monœcious plants, so that there may be a closer connection between these phenomena than is usually supposed to be the case.]

A consideration of all that has been said so far leads naturally enough to the conclusion that one of the great desiderata just now is a study of the details of the nutrition of plants, more especially what we may term the *local* nutrition. We want to know the causes which determine the amount of food that goes to the various parts—the vegetative organs (including such as are detached for

reproductive purposes), the various flowers upon an inflorescence, the different parts of the same flower, the fruit, etc. The amount of material available to the plant is not unlimited, but depends upon the light-energy available and the amount and efficiency of the chlorophyll-apparatus. If a large expenditure be made in one direction, the plant must retrench in another. A beginning has been made in this direction by Vöchting's work, already mentioned, and by that of Burkill and Tansley upon the variation in number of the parts of the flower. In flowers such as Ranunculaceæ and Rosaceæ with indefinite stamens and carpels, the number of these decreases on the whole in passing up a raceme or down a cyme, and from the beginning to the end of the flowering season.

Of the remaining phenomena which come under this heading, little need be said. We are no nearer, apparently, to an understanding of the riddle of heterostylism than when Darwin's book appeared. Burck has shown that the Cassias, whose flowers have the style bent either to right or left, are really self-fertilising, so that Müller's idea that we have here a parallel case to ordinary heterostylism, must be given up. Müller is also responsible for another fanciful "adaptation"—the "sun" and "shade" forms of *Euphrasia*, *Lysimachia*, etc. He endeavoured to prove that the plants growing in shady places had smaller flowers, better adapted to self-fertilisation, than those in sunny spots. Whilst on the whole this has some truth in it, it cannot be called an adaptation, for we find both forms almost equally common in either situation, along with every possible intermediate condition. It seems in fact to be more a case of individual variability than anything else.

THE COLOURS OF FLOWERS.

These are usually regarded as being adaptations to insect visitors. Amongst the highest groups of insects, such as the butterflies, bees, drone-flies (Syrphidæ), etc., there is no doubt a considerable sense of colour difference, as Müller and others have pointed out. But among the lower groups it is rather doubtful if such is the case. The more

that observations upon flowers and their insect visitors multiply, the less certain seems the evidence of a colour preference among the lower classes of insects. If this be so, it is evident, since the evolution of flowers has gone hand in hand with that of insects, that only in comparatively recent times can the latter have produced any marked effect upon the colours of flowers, and that too mainly in the higher (biological) groups of flowers. Many wind-fertilised flowers, *e.g.*, *Rumex*, *Corylus*, the male flowers of many conifers, the females of *Abies*, etc., exhibit brilliant colouring. So also do the reproductive organs of many cryptogams.

Again we usually find it stated that the lines and spots of different colours, found upon the corollas of so many flowers, are adaptations to insect visitors—honey-guides, to use Sprengel's term. That they have a value in this way is pretty certain, but variegation is not uncommon in leaves or fruits, and is even found in flowers at points where it can have no reference to the honey. Flowers are very common that possess honey but have no guides, and *vice versâ*. There are many other interesting facts connected with colour in flowers, *e.g.*, the change of colour with age shown by *Diervilla* (*Weigelia*), many Boraginaceæ, and other flowers. This has been stated to be an adaptation (to show clever insects which flowers are not worth visiting), but is far more probably a simple chemical phenomenon, comparable to the colour changes seen in leaves and elsewhere. Another curious point is the three different colours (red, white, and blue), shown by the flowers of the common milkwort (*Polygala*). This seems to have some connection with the constitution of the soil, and also with the local nutrition, as all three sometimes occur on one plant. Sufficient, perhaps, has been said to show that we are far indeed from understanding the phenomena of colour in flowers, and that only secondarily and in comparatively few flowers can they be regarded as adaptations to insects. There is much to indicate that colour phenomena are largely bound up with sex and reproduction; the chemical changes occurring in the preparation of the reproductive and

sexual cells may perhaps often cause colour phenomena as mere "bye-products". The phenomena of chemotaxis, movement in response to chemical stimulus, seem to indicate a chemical difference between male and female cells, and we need not therefore be surprised at a difference in colour between male and female flowers such as is often seen.

DICHOGAMY.

When stigma and pollen do not ripen simultaneously the flower is said to be dichogamous; when they do, it is homogamous. If the stigma be first ripe, the flower is protogynous; if the pollen, protandrous. The multiplication of observations in various localities goes to show that this phenomenon also is very variable. In some regions a plant may be highly protandrous, in others only slightly so, or it may even be homogamous or protogynous. It has also been observed to vary with the season of the year.

FLOWERS AND THEIR INSECT VISITORS.

In this department of the subject an enormous mass of material has been collected. The literature relating to the European flora has all been collected and published in abstract by Loew. The results support the Müllerian theory of flowers very well, and are of interest in showing the great differences in the floral phenomena between different localities, even in plants of the same species. This point we have already considered.

A commencement (4) has been made upon the British flora, with the result of showing that short-tongued flies perform to a large extent the work done by the smaller bees, etc., upon the continent. MacLeod has worked out the flora of Flanders in a most thorough way, and a good deal of other similar work has been published.

MacLeod has also investigated the annual development of the chief groups both of flowers and insects in Flanders. For each natural order of flowers the number of species in flower in each month was noted; the resulting curve exhibits in each case a gradual rise to a maximum and a

steady fall. The larger insect groups (coleoptera, long-tongued flies, etc.) were treated in a similar way, the number of species of each that visited flowers being noted, but the curves obtained were never so regular, and in some cases went up and down in a most erratic way. It was shown however that the groups of insects have their chief maxima at different times, *e.g.*, the Lepidoptera in July, the short-tongued bees early in the season, and so on. Other papers of the same nature have also been published lately.

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The literature of this subject is very fully indexed in Müller's *Fert. of Flrs.* (to 1882), in MacLeod's paper in *Botan. Jaarb.*, 1890 (to 1889), and in Loew's book (to 1894), and other books and papers above mentioned.

JOHN C. WILLIS.

PRESENT KNOWLEDGE OF THE MECHANICAL TESTING OF IRON AND STEEL.

ENGINEERS have long recognised the need of testing the materials employed in the machinery and structures they construct, but it is undoubtedly owing to the rapid development of new materials that the question of testing has come so much to the front in recent years. Since 1852, when the first Werder machine was constructed for the Railway Commission of Bavaria, the advance has been extremely rapid, and the accuracy of the machines now at the disposal of the engineer has led to more thorough and complete methods of testing; errors have been eliminated, until at the present time the scientific testing of the strength and other properties of materials is carried out in as exact and careful a manner as in any other physical determinations.

This has largely been brought about by the establishment of state testing stations in many places on the Continent, all of them equipped with the most perfect appliances and under the direction of most able investigators; the publications issued from time to time from them are perfect store-houses of information on this subject.

The establishment of such testing stations in this country is perhaps neither desirable nor necessary, but it is decidedly a pity that the great technical societies have not, with one exception, used their funds more generously in providing the money necessary for research on many of the yet unsolved problems of the strength of materials. There are now several colleges in Great Britain possessing admirably organised laboratories under the charge of men capable of doing good work in this direction, but hitherto their work has been cramped for lack of funds to carry out what must, from the nature of the case, be often most expensive experiments. It can hardly be expected that the colleges can provide from their generally very scanty funds the grants needed for such purposes; it is often as much as

they can do to keep up the regular teaching work of the laboratories.

The field covered by the mechanical testing of iron and steel is too wide to be fully dealt with in this article; it is proposed therefore to confine attention to some of the more important points, to briefly state the extent of our present knowledge on them and to indicate the lines upon which it is desirable that further advance should be made.

Forms of Test Bars. It is certainly a matter of great regret that some system of uniform proportions for specimens for testing has not been adopted. As the matter now stands it is often extremely difficult to compare the results obtained by different experimenters, especially in the case of tensile tests when dealing with what is usually termed the ultimate extension.

In hard materials this difficulty does not arise, because after the limit of elasticity has been passed there does not occur to any extent that remarkable change in the molecular condition of the bar which is so pronounced in the ductile irons and steels, the bar in fact does not pass into the semi-plastic condition.

With ductile materials, however, as soon as the Yield Point has been reached the material passes into the semi-plastic condition, and just before rupture occurs we are confronted with a condition which most materially alters the ultimate extension. Up to the point in the test at which this occurs, it is when the maximum load which can be carried is reached, the extension has been fairly uniform all along the bar; if the bar broke at this stage the percentage extensions of all bars would no doubt be very strictly comparable, the extension being then proportional only to the length of the original bar for material of the same quality. But as soon as the maximum load is reached the extension becomes local, and the specimen draws down at the point at which fracture finally occurs; occasionally it draws down also at some other point to a certain extent. This local extension being confined to a very short length increases the ultimate extension of a short test piece proportionately much more than a long one.

M. Barba enunciated some years ago certain laws in regard to this point. He stated that in cylindrical test pieces of similar form the percentages of extension at fracture are the same for similar material, and that in the case of bars of the same length but varying in their cross-sectional area, or of bars of the same cross-sectional area but varying in their length, the percentages of extension vary as the ratio of length to diameter, and decrease as this ratio increases.

These laws have been very fully investigated by Mr. L. H. Appleby in a paper contributed to the *Proceedings of the Institution of Civil Engineers* (vol. cxviii., p. 395), embodying the results of a long series of experiments carried out at University College, London. The materials experimented with were two qualities of Bessemer steel, a hard and a soft quality, both specially prepared for the experiments. Briefly, the results of the experiments may be said to conclusively verify Barba's laws. They also showed very distinctly that the local contraction before fracture was enormously affected by local hardness or impurities, a point before observed by Bauschinger and others.

There can be little doubt that much better results for the sake of comparison, and even for judging the quality of a given material, would be obtained, if the final extension on some fixed short length which includes the fracture were discarded, and only the rest of the ultimate extension given when recording any experiment. Probably the most convenient standard for cylindrical bars would be obtained by making the ratio of length to diameter 10. The whole question was very fully thrashed out as regards English engineers, in the discussion on the late Mr. Hackney's paper on "Forms of Test Pieces" (*Inst. Civil Engrs. Proceedings*, vol. lxxvi., p. 70); it was then pointed out that there were great practical difficulties in the way of the adoption of a fixed ratio of length to diameter, owing to the fact that in testing gun steel, tire steel, etc., the specimens must of necessity be made very short, hence with a fixed ratio of length to diameter such as 10, the cross-sectional dimensions of these specimens would be unsuitably small.

Where, however, tests are made of bars of iron and steel, and where there is no question of this difficulty, it would be a great convenience in making comparisons if a standard value for the above ratio were adhered to. The German system has been to make the cylindrical test bars always 10 diameters long; in recording the extensions they also give the extension on only one half the original length, taking the point of fracture as the central point of this length. For flat specimens the ratio of width to length is 1 to 5, and the natural thickness of the plate or bar is adhered to up to about three-quarters of an inch, above this round specimens of standard dimensions are turned out of the material.

The Admiralty length for steel plate specimens is 8 inches, and according to the German rule this would be accompanied by a width of 1·6 inches, a reasonable and practical value.

There is no necessity to adhere slavishly to such rules, but it does seem absurd to put aside the obvious advantages to be obtained by working on these lines whenever it is possible.

Reduction of Area at Fracture. Many experimenters and engineers lay great stress on the value of this quantity as a guide in estimating the ductility of a material, and undoubtedly if taken along with the condition of the fractured surface it is of much value. But as was pointed out by Bauschinger, and shown very clearly by his museum of specimens, a comparatively minute flaw or hard spot at the point of fracture would frequently diminish the reduction of area to an extent apparently far in excess of the proportion of the flaw to the rest of the fracture surface. Similarly with flat specimens, imperfectly formed ends, though they influence but little the maximum load carried, or the ultimate extension, often seriously effect the reduction of area, sometimes making it apparently almost zero.

It cannot, therefore, be contended that this factor is such a reliable guide as the ultimate extension in judging the quality of a material. It is well, however, to dispel a statement often put forward against the value of the figures recorded

for the reduction of area in tests. The author has measured the reduction of area of thousands of specimens, and never, or only in the case of exceptionally badly welded wrought iron specimens, experienced any difficulty in making perfectly accurate measurements of the reduced dimensions. The reduction of area, in ninety-nine cases out of a hundred, can, with the help of vernier callipers, be measured quite as accurately as the ultimate extension ; the latter, in the case of irregular fractures owing to the difficulty of fitting the two halves together, cannot very often be determined with accuracy. The objection, therefore, to measuring and recording this factor owing to these supposed difficulties falls to the ground ; in the few cases where it is impossible to do it the material is usually too defective to pass muster.

As to whether it is a fair test of the ductility of the material is quite another matter, but it certainly does materially help one in coming to a conclusion.

It is often stated that the percentage of reduction of area at the point of fracture rarely exhibits any relation to the percentage of ultimate extension, and that for this reason it is unadvisable to use it. For a perfectly plastic material it can readily be shown that the percentage contraction of area is directly proportional to the percentage of elongation, expressed however not in terms of the original length but of the final length. Now no test piece is truly plastic in the latter part of the test except, perhaps, in the immediate neighbourhood of the place of fracture, and therefore it would be impossible for any direct relation to hold between these two quantities, when the extension refers to the whole bar. If, however, the percentage of elongation is obtained for a short length only, which includes the fracture, say for one inch of the original length at that point, and this can always be done if the whole bar has been marked off in inch lengths prior to the test, then it will be found that this quantity is usually in close agreement with the percentage reduction of area, because we are now dealing only with the portion of the bar which is very nearly truly plastic. In my own testing work I always have the bars marked off into inch lengths, and make these measurements which I find of great value.

Since this involves but very few extra measurements and very little time, in fact they can all be made and the results worked out while the next bar is being fitted into the machine, it is a pity they are not systematically recorded in all tests.

Work of Fracture. Professor Kennedy pointed out in a discussion at the Institute of Civil Engineers (*Proceedings*, vol. lxix., p. 30), that the amount of work spent in fracturing any bar, expressed in inch tons per cubic inch of the bar, was a most valuable indication as to the power of any given material to resist sudden shocks or blows. Whenever an autographic stress-strain diagram has been drawn during the fracture of a bar it is an easy matter to calculate the work of fracture, since it is proportional to the area enclosed between the stress-strain curve and the axis on which the strains are set off, and its numerical value is known directly we know this area in square inches and the scales of stresses and strains ; it can also be similarly determined when the curve has been plotted afterwards from the observed strains recorded during a test. But as both these operations involve a good deal of labour and time, it has generally been assumed that the determination of this factor was beyond the scope of ordinary testing. Professor Kennedy, however, in this discussion gave a formula for obtaining the work done in fracture, involving only a very short arithmetical calculation ; he pointed out that all the data required in his formula were known from the observations always made during an ordinary test. The formula is that the work done in inch tons $= \frac{wx \,.\, (r + 2)}{3}$, where w is the maximum load carried in tons per square inch, x is the final extension in inches and r the ratio of the Limit of Elasticity to the Maximum load. By Limit is here meant the more scientifically termed Yield Point or break-down point, which is always readily enough indicated during the test by the steelyard drop and therefore involves no delicate instrument for measurement. All the three factors of the formula are known directly the test is complete, and with the aid of a slide rule the simple calculation required is made in a few moments without causing

any delay in the work of testing. No other result obtained in a tensile test shows more clearly, for instance, the utter untrustworthiness of wrought iron plate when stressed across the grain if it is liable to shocks or sudden loads, or shows more distinctly the superiority of steel plate in this respect.

Limit of Elasticity. No term has given rise to more confusion in dealing with the strength of materials than this. One instance will suffice to show the kind of error produced by false notions as to its meaning; the well-known fact that by stressing a bar in tension beyond its Yield Point we raised its Limit of Elasticity in Tension was usually considered to represent the whole result of the action. We know, however, from the researches of Bauschinger that this is a very imperfect and misleading conclusion, a conclusion, too, which may have caused much of the difficulty in understanding some of the results obtained in the endurance tests of Wöhler.

Bauschinger's experiments, carried out with perhaps more exactitude than any previous experiments of this nature, lead us to conclude that there is for any given material a true natural Limit of Elasticity, understanding by that term the Limit of the Elastic Condition according to Hooke's law, *ut tensio sic vis.* But this natural Limit may be varied in all sorts of ways, by strains set up in the material during manufacture, by after working, or in a testing machine. Unless, therefore, we know the whole previous history of the bar we are testing we are quite unable to say whether the Limit obtained in our test is a natural one, or whether it is some artificial one produced by the treatment the bar has undergone during manufacture or afterwards. In one form this fact was appreciated by all those interested in the question of testing; it was well known, for example, that cold rolling greatly raised the Limit, two bars rolled from the same ingot would give very different values for this factor, the one with the smaller cross-sectional area and therefore during the last stages of manufacture rolled while partially cold would show a much higher apparent Limit. But it was not, at any rate generally, understood that such effects were only particular cases of a much

more general law. This fact at once disposes of much of the talk often indulged in during discussions on this subject as to the value of high elastic limits when they are determined simply from a static tensile test; and the importance of the question is at once realised when we consider that these so-called higher values are made the basis of a claim for higher working stresses, even for cases where the loads are alternating tension and compression.

It is to be hoped, therefore, that this question of the determination of the Natural Limits of Elasticity of different qualities of iron and steel will be undertaken as a research work by some competent observers having the requisite delicate measuring apparatus at their disposal. Such a research if carefully carried through would settle many important points of the utmost practical value, and would at the same time confirm experiments which, from their far-reaching consequences, and from the fact that the deductions from them depend on extremely small differences in measurements, themselves very minute, much need confirmation.

Carried out in a systematic manner, careful chemical analysis being made of the material tested, we should obtain data to make it possible to deduce the relations existing between the values of the natural limits, and the proportions of the various constituents of other elements present in iron or steel, and probably be able to ascertain definitely the best proportions for a material required for some given purpose. It would also enable us to settle in a much more rational manner the working stresses which could be allowed in any given case; this, a matter of the utmost consequence in the case of parts of machines or structures subjected to alternations of stresses, is equally of importance where there are only variation of stresses or even only long-continued statical loads, for we are still without information as to the changes which may go on in the value of the Limit under these conditions.

That a steady change in the opinion of engineers in regard to this question of working stress has been in progress for the past few years is undoubted, but at present we are still groping more or less in the dark, because though

there is a mass of rapidly accumulating data, it at present seems largely to be only capable of being used in a more or less empirical fashion and still to be in need of some rational explanation. The researches of the Committee of the Institution of Mechanical Engineers on "Alloys" has no doubt done something to shift the question into a clearer light, but the field open for research is still very wide.

Repetition of Stress. The questions of the value of the natural limit and of safe working stresses are intimately bound up with that of the effect of repeated loads on the endurance of materials. Many engineers, taught by experience, often costly, had realised the importance of this question before Wöhler's classical experiments drew the attention of every one to this vital point. It is, therefore, to further research in this field, combined with the experimental work that I have just alluded to, that we must look for a truer insight into many obscure questions, and for the explanation of many apparently inexplicable failures and break-downs. It is, however, a most costly line of work, and one which from its character can only be adequately carried out, when funds are forthcoming either from the State or from the Technical Institutions interested in the question. The machinery needed is purely special, and must be continually at work, and it involves the use of a not inconsiderable amount of power constantly running, in addition to the time and attention of observers who must be largely free from other duties.

There is one difficulty in connection with this subject which is perhaps not sufficiently attended to in experiments on the effect of repeated loads, namely, in the determination of the actual range of stress in any given case. This can in most cases only be determined by certain calculations, which generally ignore, or consider as non-existent, effects due to vibrations set up by the instantaneous application and removal of the loads, or to inertia of moving parts of the machine. These may no doubt with carefully designed appliances be practically eliminated, but the first of them at any rate may possibly quite unexpectedly creep in, and in an experiment of this nature a very small error may

produce very serious results, perhaps deciding into which of two important groups of results a particular experiment is to be placed.

When it is remembered, that apart from inertia effects, the instantaneous stress produced by a sudden load, if it does not exceed the limit, may be twice as great as the static stress produced by the same load when applied gradually, it will be conceded how important the point is in such calculations; the real stress due to the load may be well within the limit of natural elasticity, and yet the instantaneous stress set up may exceed it, and thereby when often enough repeated seriously change the molecular condition of the bar.

This point is dwelt upon, because it seems to be too often forgotten when discussing the effects of repeated loads, and the results they produce in parts of machines or structures, and also in estimating the so-called factor of safety.

The lines therefore, to sum up, upon which further scientific testing should proceed are two: (*a*) The determination of the real natural Limits of Elasticity of different qualities of iron and steel, and the influence upon it of the various chemical constituents, and the way in which it is altered by various methods of treatment, that is, the question of artificially raised or lowered limits. In this connection it may be interesting to remember that the Modulus of Direct Elasticity is the one function which seems most constant, no matter how the quality of the iron or steel varies, still it does seem to change as the carbon percentage is increased, being gradually diminished in value as that percentage rises. May it not, therefore, turn out that there is a direct connection between the values of the natural Limit of Elasticity and the Modulus of Elasticity?

(*b*) The study of the effect of repeated loads, continued on the lines of Wöhler, Spangenberg, Bauschinger, and others, and in particular the solution of the question as to what is the molecular change induced by repeated loading beyond the range of the Limit of Elasticity, and why it is that the broken parts of the bar when tested at once in an ordinary testing machine show apparently no molecular

change comparing them with material which has never been stressed at all. Until this obscure point is cleared up we shall never be able to understand thoroughly this question.

Commercial Testing. In concluding this brief notice of the present condition of testing iron and steel, it may be worth while to state what it is desirable that the commercial testing, carried out only to determine whether the quality of a given parcel of material is up to the standard, should aim at. There is no doubt that simultaneously with the tensile tests what may for convenience be termed the workshop tests should always be rigorously insisted on, namely, for tires, axles, rails, etc., the drop tests; and for plates, bars, etc., the bending tests. These tests do at times detect faults which would not so readily be brought to light by the simple tensile tests of samples of the material only, but they should always be accompanied by the latter test. If they are properly carried out and an accurate record of the results obtained is kept they must throw much light on many important questions, but if, as is so often the case, they are done in a careless fashion with no precautions to eliminate possible errors, they will probably simply produce results so discordant as to be useless, and puzzling and unsatisfactory to both producer and user.

I have already dealt to some extent with the question of the form of test bars. Personally I would advocate for all flat specimens a uniform length of 8 inches in the parallel part, and a width of 1·6 inches for all thicknesses up to 1 inch; beyond this it is better to have turned specimens. The ends should if possible be at least 2 inches wide and long enough to secure a good grip of the wedges, though for moderate thicknesses perfectly plane parallel strips will give quite trustworthy results. For all ordinary turned specimens a ratio of length to diameter of 10, but in the case of tires, axles, gun steel, etc., a ratio of $2\frac{1}{2}$ to 1 will probably be found best, the ends being formed either with a collar to pull from or screwed. Though for scientific work it is advisable to pull through a spherical bearing, it is not of much consequence for ordinary work; the ends must be exceptionally badly formed and held to

produce any appreciable effect on the results recorded in such testing.

Should the metric system become compulsory it would certainly be advisable to try and obtain an international agreement as to the leading dimensions for ordinary test bars.

Next as to the observations which should be carried out and recorded. Every test record sheet should have a column in which could be briefly recorded the past treatment of the specimen, such as whether it had been turned down or forged down to size, whether sheared off, annealed, and so forth, for without some such brief history it is almost impossible to properly interpret the test records.

The original dimensions should be given, the Yield Point as determined by the steelyard drop, and the maximum load carried with the ratio of these two, both of course expressed in pounds or tons on the square inch ; also the whole extension expressed as a percentage of the whole original length, and the extension on the two inches or one inch which includes the point of fracture (according to the original length of the bar) also expressed as a percentage ; lastly, the reduction of area per cent., meaning by this the ratio of the lost area to the original. These observations are perfectly easily recorded in every test, and with no more time spent, as repeated experience has convinced me, than when some of them are omitted. Finally, I would have the work spent in fracturing the bar, calculated as above, per cubic inch, entered on the record sheet, and a brief note as to the condition of the fractured surfaces.

With figures such as these before him from the test house, with similarly carefully recorded results of the impact and bending tests, no engineer would have the least difficulty in coming to a decision as to the value of the material the maker was supplying to him, nor would he ever have the slightest future cause for anxiety in regard to any material which had successfully passed the ordeal. Checked by the chemical analysis the maker too would know the lines on which he was working, and how and where he could improve the quality of the material he was putting on

the market, or how he could produce something which would come up to the particular standard fixed for some special work.

It is most desirable too that there should be a few machines in every country capable of dealing with full-sized members of structures, such as built up struts, tiebars, etc., this has been too much neglected in Great Britain; in regard to full-sized struts we are in need of much more detailed experiments before anything can be done to put the empirical formulæ now in use on a more satisfactory footing. In fact, as a rule, compressive tests to be of real value must be made on the column or strut of the full size, or at any rate as near to it as possible, since it is almost impossible to deduce very satisfactory conclusions from the results of the tests of mere specimens.

Space does not permit me to deal with the equally important question of temperature effects on the resistance of iron and steel to stress, especially the effect of long-continued alternations of temperature and stress. Much good work has been done by Mr. Andrews and others, but much still needs to be done if we are to determine accurately the Physical Laws governing these facts.

T. Hudson Beare.

LOUIS PASTEUR.

LOUIS PASTEUR, one of the scientific leaders of the century, has passed from amongst us. His personal work is finished, and though the influence which this work will have on the progress of science during the next century cannot at the present time be calculated or foretold, even approximately, we already know how far his investigations have extended our knowledge of Molecular Physics, altered our ideas concerning certain biological problems, and revolutionised our conceptions of the Ætiology and Pathology of the specific infective diseases. It may be held by some, as Pasteur himself held, that the only record of his work that need be kept is that contained in the *Comptes Rendus* of the Academy of Sciences of Paris, but we are glad to say that a sketch of the life of the man who carried on the great work now inseparably associated with his name has been given to the world by his son-in-law, M. Valery Radot, under the title *M. Pasteur, Histoire d'un savant par un ignorant.*[1] For many of the facts of Pasteur's life I have drawn freely on this sketch, but the source from which most of the facts relating to his work are taken is that to which he himself would refer his followers—the *Comptes Rendus.*

Like so many of the greatest sons of France, Louis Pasteur was born, not in one of her great cities, but in Dôle in the Jura, the ancient capital of *Franche Comptes*, and originally the seat of the University College now at Besançon in the Doubs. His father,—a quiet, contemplative, and studious tanner was a retired soldier who for his valour had been decorated on the field of battle,—married an enthusiastic and ambitious woman, ambitious not for herself or her husband, but for the future of the son who was born to them on the 27th December, 1822. Father and mother alike determined to make of their son an "educated man"; what success they

[1] Translated from the French by Lady Claud Hamilton: *Louis Pasteur, His Life and Work.*

attained we are now in the best position to form an opinion. Three years after the birth of this son the Pasteur family removed to Arbois, in the valley of the Cuisance, so noted for its wine, where the lad received his early education in the Communal College, the father devoting his evenings to the superintendence of his son's lessons. Like most clever lads he was not noted for his studiousness at this period, fishing and portrait sketching occupying some of the time and energy that might perhaps have been devoted (with less advantage to the boy probably) to school work. Some of the sketches made at this time, when he was only thirteen, were so clever that it was afterwards said of him by an old lady friend: "What a pity that he should have buried himself in chemistry. He has missed his vocation, for he might by this time have made his reputation as a painter." However, Pasteur had a talent for work which could not long lie dormant, and he very soon set himself seriously to master all that could be taught him by his teachers. In order to complete his studies in Philosophy, Pasteur went from Arbois, where his interest in chemical study had been kindled by M. Darlay, to the College of Besançon; here, at the end of his scholar's year, he received the degree of *bachelier-es-lettres*, and was appointed tutor in the college; he was thus able to continue his studies for the entrance examination at the École Normale. In 1842 he passed fourteenth, but determined to devote another year to study for the same examination, with the result that in October, 1843, he was placed fourth on the list of successful candidates. He at once commenced to study chemistry at the École Normale under M. Balard, and at the Sorbonne under M. Dumas, from the former of whom he learned to be enthusiastic and voracious for facts, from the latter a love for precision and exact experiment.

Here, with leisure to read and every encouragement to carry on independent investigation, Pasteur now laid the foundation of his future work, and, under the encouraging direction of M. Delafosse, devoted himself to the study of Crystallography, especially in relation to the arrangement of the molecules in these crystals. As Tyndall

suggests, probably bearing in mind the properties of rock crystal, and the difference of the facets in the right- and left-handed crystals, he saw reason in Mitscherlich's note to the Academy of Sciences—that tartrates of soda and ammonia caused the plane of polarised light to rotate to the right, whilst the paratartrate, which in chemical composition is absolutely the same, causes no rotation—to make a careful search for facets like those that had already been found to exist in rock crystal. Such facets he was able to find in the tartrates, and this being done he assumed that the want of power of rotating light which existed in the paratartrate must be due to the combination of the opposite rotating powers of right- and left-handed crystals. He anticipated, indeed, that the tartrate was without a plane of symmetry, whilst the paratartrate was symmetrical, and that the crystals of the paratartrate would be all characterised by the absence of dissymmetry; on coming to examine the crystals carefully he found, however, that certain of them were dissymmetrical in one way, others in an opposite. "Some of these crystals when placed before a mirror produced the image of the others, and one of the two kinds of crystals corresponded rigorously in form to the tartrate prepared by means of the tartaric acid of the grape," and he argued that one of these forms of crystals must therefore be the one found in the right-handed tartrate, whilst the other must be something different, and he undertook the laborious task of separating the different kinds of tartrate crystals, placing them in two groups: those like the ordinary right-handed tartrate, and those which he found rotated light to the left. These experiments were carried out during his final year of study in the École Normale where he took his Doctorate in 1847, during the time that he was Professor of Physical Science in the Lycée of Dijon, and whilst he was acting as Assistant Professor of Chemistry in the University of Strasburg. (It was in Strasburg that Pasteur met his future wife—Mdlle. Marie Laurent.)

As a result of these physical and chemical observations Pasteur conceived the idea that there was an essential differ-

ence as regards symmetry between the products of inorganic nature and organic products, the one set having a superposable image, therefore being not dissymmetrical, the others having an image not superposable, and being therefore atomically dissymmetrical, "this dissymmetry expressing itself externally in the power of turning the plane of polarisation".

In 1854 Pasteur was nominated Dean of the Faculty of Sciences at Lille, and a new series of investigations was commenced.

It is curious that such a purely physical question as that above outlined should be the real foundation of Pasteur's contributions to the solution of some of the great biological problems that at this time were occupying the minds of scientific men. It was already known that impure tartrate of lime contaminated with organic matter and kept in water would undergo a process of fermentation during which various products were yielded. In order to examine this process Pasteur took about one hundred parts of the pure crystalline right-handed tartrate of ammonia, dissolved it, and added to the solution one part of albuminoid matter. He found that when this was placed in a warm chamber fermentation took place, and the clear solution became turbid, this turbidity being due to the appearance of a number of minute organisms which, following up the analogy of the presence of yeasts in alcoholic fermentation, he assumed must be the cause of this special form of fermentation. He then obtained similar fermentation in a solution of paratartrate of ammonia, when he found that the same organism still made its appearance. Following one of the changes that took place in the fermentation of the two salts with the aid of the polariscope he found that in the case of the paratartrate, rotation of light to the left could be demonstrated at an early age of the process, from which it was evident that the right-handed tartrate only was being attacked and broken down under the process of fermentation; this was allowed to go on until the deviation to the left had attained a maximum, "the fermentation was then suspended, there was no longer any of the right-handed acid in the liquid, which, when evaporated and mixed with its own volume of alcohol, immediately furnished a

beautiful crystallisation of left-handed tartrate of ammonia". If, however, he allowed the fermentation to continue beyond this point the whole of the tartrate was broken down. It was evident, then, that in this case these minute organisms have a power of selecting the special form of tartaric acid and that they have a preference for this, although, when this special form is used up, they fall back on the other and break it down in the same way as they have already used up the first.

In continuing these experiments, Pasteur made the first of that series of experiments by which he was able to corroborate Schwann's experiments designed to prove that no life could be generated from dead organic matter, but that, if certain organisms gained access, even to special forms of inorganic matter, they were capable of multiplying freely ; he was able to show that the spores of the Penicillium glaucum, or common mould, could grow and produce the fully formed mould, in a solution of paratartaric acid—to which nothing but phosphates of potash, of magnesia, and an ammoniacal salt of an organic acid had been added—first using up the right-handed tartrate and then attacking the left.

Liebig and Gay Lussac had both taught that fermentation and decomposition were due to the action of the oxygen of the air upon nitrogenous substances, and that this process was due to the initiation of certain molecular movements by this process of oxidation, these movements being gradually communicated from particle to particle of the fermentable substance, which was thus resolved into the products of fermentation. Others—Berzelius and Mitscherlich—explained fermentation as the result of the action of a nitrogenous ferment which exerted a catalytic action by its mere presence, adding nothing to the solution and taking nothing from it, but exerting its influence by what these philosophers termed the phenomena of contact. Pasteur was satisfied with neither explanation. Caignard-Latour and Dumas—Pasteur's teacher—both associated the growth of the yeast-cell in organic solutions with the process of fermentation that accompanied this growth.

Pasteur made his first experiments on fermentation with milk, in which he recognised, as the process of fermentation went on, the lactic ferment, which he described as little rods with slightly constricted centres, so small that their diameter was scarcely the twenty-five thousandth of an inch. This rod multiplied by a process of division, each one into two shorter rods which, in turn, assumed the shape and size of the original. To isolate this organism and to obtain a comparatively pure culture, Pasteur boiled a little yeast with from fifteen to twenty times its weight of water; he then carefully filtered the liquid, dissolved in it about 5 per cent. of sugar, and then added some chalk. This he seeded with a trace of the ordinary lactic fermentation deposit, with the result that the next day a lactic acid fermentation had set in, the liquid became turbid, the chalk disappeared, and a deposit of lactic ferment gradually took its place. He then repeated his previous experiment of adding this special ferment to small quantities of crystallisable salt of ammonia and phosphates of potash and magnesia. In this he found also, that although there was no organic matter present beyond that which he had introduced in the shape of his organisms, they still continued to multiply and to give rise to the typical fermentation, both yeast and the lactic ferment acting in the same way, each giving rise to its own special product. By these experiments both Liebig's and Berzelius's theories were at once shattered, as the carbon was supplied by the fermentable substance, whilst the process of fermentation could not be due to the action of oxygen on nitrogenous substances, as nitrogenous matter did not enter into the constituents of the solution, and still a vigorous fermentation had taken place. Pasteur concluded that fermentation was simply a phenomenon of nutrition—the nutrient substance being the sugar, the organism nourished being the ferment germs,—whether they were lactic acid organisms or yeast—the organisms contriving to obtain the materials necessary for the building up of their complex organisation from the sugar and the purely mineral elements.

Then followed the discovery of the butyric ferment,

which, unlike the lactic ferment, in so far as the organism by which it is brought about,—which Pasteur was enabled to separate,—exhibits the greatest activity in the absence of oxygen ; indeed, when a current of air was passed through the fermenting liquid, the organism—a vibrio—became motionless, fell to the bottom of the fluid, and the process of fermentation ceased. These two organisms were taken by Pasteur to represent two classes : (1) the *aérobies* or organisms which require a supply of free oxygen in order that they can live and carry on their fermentation function, and (2) the *anaérobies* which not only can live without air but flourish best in this condition.

In both cases the organism uses up for its own nutrition a comparatively small amount of the fermentable substance, but decomposes a much greater quantity of this material, breaking it down into what are known as the products of fermentation. He found, however, that yeast could bring about fermentation not only in the presence of oxygen, but also when it was absent, although the amount of decomposition was very different in the two cases. Where no air has access to the organism, a single part of yeast will break down from 100 to 150 parts of sugar ; whilst when oxygen is present it will only break down something like five or six parts, and Pasteur pointed out that in the one case the organism obtains a large part of the oxygen necessary for its growth directly from the air, and therefore does not require to break down so much sugar in order to obtain the elements that it requires for its nutrition ; whilst, when air is absent, the oxygen must be obtained from the sugar, as a result of which a very large amount of alcohol is produced during the rearrangement of the molecules rendered necessary by the abstraction of the oxygen.

Various forms of putrefactive processes were then studied by Pasteur, who was able to show that they depended upon organisms similar to those that he had already described in the processes of fermentation, that some of these were aerobic, some anaerobic, that they differed in character according to (1) the nature of the medium to be broken up, and (2) the nature of the organism that was engaged in

setting up the special fermentation. He pointed out, too, that one organism would carry on the process of decomposition to a certain stage, whilst another organism would carry the process a stage farther, so that ultimately the most complex bodies are broken down through the agency of these putrefactive and fermentative organisms into the simplest products, ammonia, carbonic acid and water, the anaerobic organisms doing one kind of work, the aerobic organisms another. He demonstrated that through their agency all organic matter, whether animal or vegetable, as soon as it loses its power of assimilating—in fact, as soon as it dies and becomes effete—is broken down into substances which may be assimilated by plants, living organisms in this way being enabled to live on the dead material that had been stored up by previous generations of these same living organisms.

Pasteur's investigations now, for a time, took a practical turn. The vinegar manufacture, upon which Orleans depends very largely for its industries, claimed his attention. The old process was tedious, uneconomical, and precarious. Pasteur, working at the subject, found that a pure culture of the *Mycoderma aceti* has the power of taking up oxygen and of transferring it to the alcohol of wine, through which process such alcohol is turned into vinegar. He therefore advised that a pure culture of the *Mycoderma aceti* should be sown in a mixture of wine and vinegar, and that this should be kept at a temperature of 20° or 25° C. At the end of a couple of days the *Mycoderma* covers the whole liquid, and in eight or ten days this is converted into vinegar, the process commencing immediately the plant begins to grow. If, however, the process is allowed to go on too far the vinegar is still further broken down, and ultimately every trace of acetic acid may disappear, this being the result of the action especially of certain anaerobic organisms which continue the process of breaking down, so that ultimately nothing but a foul or odourless and tasteless fluid may remain, each step appearing to be the result of one of these processes to which reference has already been made. These researches gave a new life to the Orleans' vinegar manu-

facture, and at the same time enabled Pasteur to demonstrate that his theory of fermentation, and not that of Liebig, was undoubtedly the correct one.

To-day it seems almost impossible that only so recently as a little over twenty years ago, the theory of spontaneous generation should have been thoroughly believed in, and that ingenious experiments were made and great ability displayed in defence of this theory as regards the development of living bacteria. Van Helmont's evolution of mice from a pot charged with corn and stuffed with a dirty shirt, and his statement that the smells which arise from the bottom of morasses produce frogs, slugs, leeches, grasses, and other things, are now looked upon as scarcely more extraordinary, although they date back a couple of hundred years.

Having studied the process of fermentation and noted the relation of effect to cause, and having also noticed that the commencement of fermentation and putrefaction processes were invariably associated with the process of seeding out, through the deposition of living organic particles in his nutrient media, he naturally—I say naturally because Pasteur was the observer and logician who was investigating the point under consideration—came to the conclusion that living organisms like all animal and vegetable cells were developed only by a process of reproduction or division of the parent organisms. So many difficulties did the problem "Abiogenesis" appear to present, that when Pasteur informed M. Biot that he intended to study this question, the latter assured him "you will never escape from it, you will only lose your time," whilst his teacher and friend, M. Dumas, said that he would not advise any one to occupy himself too long with such a subject. Pasteur repeated Schwann's experiments of boiling organic fluids and then allowing only air which had been passed over red-hot platinum, to come in contact with these fluids, with the result that no evidence of organic life made its appearance even in those which were most decomposable, such as urine. He also showed that on boiling the putrescible fluid in a flask with a long narrow sinuous neck left unclosed, the fluid will remain perfectly clear, and no growth will take place,

the moist sides of the narrow neck being sufficient to prevent the entrance to the flask of any solid particulate matter that may be suspended in the air. Such a flask may be kept in an incubator for months perfectly unchanged, but as soon as the narrow neck is nipped off, and particles are allowed to fall in, and with these particles the seeds or germs of moulds or bacteria, these organisms soon make their appearance, and the process of putrefaction goes on apace. M. Radot says, "lastly, to convince the most prejudiced minds and to leave no contradiction standing, Pasteur showed one of these bulbs with the sinuous neck which he had prepared and preserved for months and years. The bulb was covered with dust. 'Let us,' said he, 'take up a little of this outside dust on a bit of glass, porcelain, or platinum, and introduce it into the liquid; the following day you will find that the infusion, which up to this time remained perfectly clear, has become turbid, and that it behaves in the same manner as other infusions in contact with ordinary air.' If the bulb be so tilted as to cause a little drop of the clear infusion to reach the extremity of the bent part of the neck where the dust particles are arrested, and if this drop be then allowed to trickle back into the infusion, the result is the same—turbidity supervenes and the microscopic organisms are developed. Finally, if one of these bulbs which have stood the test of months and years without alteration be several times shaken violently, so that the external air shall rush into it, and if it be then placed once more in the stove, life will soon appear in it."

For his work on this subject Pasteur received the Prize of the Academy of Sciences, which was awarded to him "for his well-contrived experiments to throw new light upon the question of spontaneous generation".

Tyndall in this country repeated and modified many of Pasteur's experiments, and overthrew in the popular mind the theories that had been advanced by our countryman Needham, although at the time these had been successfully controverted in argument by Spallanzani. Dr. —now Sir William—Roberts, and Professor—now Sir Joseph—Lister performed a number of corroborative and

additional experiments, with the result that although Dr. Bastian made a most gallant fight in support of the theory of spontaneous generation or abiogenesis, and displayed wonderful ingenuity and ability in its defence, he had ultimately to succumb, and it is probable that in scientific circles Pasteur's demonstrations were more fully accepted as conclusive in this country than in France, and certainly than in Germany, although here Cohn attacked the theory of spontaneous generation with the greatest energy and success. It was at this point that Lister conceived the idea of his now famous antiseptic treatment. He argued that if these germs were necessary for fermentation and for putrefaction, and if they could only be developed from pre-existing germs, might it not be possible by means of mechanical and chemical appliances to prevent their gaining access to wounds in which they must necessarily be the cause of the putrefactive processes that were often developed in them? By the marvellous manner in which he developed methods of keeping organisms from the wounds, he was able to maintain an aseptic condition through which, in old erysipelas- and gangrene-infected wards, he was able to obtain results which compared most favourably with those obtained under the best hygienic conditions. His method revolutionised surgery, and has, directly and indirectly, been instrumental in saving an incalculable number of valuable human lives. Lister, like Pasteur, was able to apply practically for the alleviation of human suffering, the results of researches which in the hands of other men had remained of merely theoretical interest and value.

Having settled this question of a spontaneous generation, Pasteur next turned his attention to a more careful study of the processes of fermentation in wine, and to the wine diseases through which so much wine was rendered of little value and even unsaleable. His previous researches, of course, led him to suppose that the disease was due to some special kind of fermentation, the result of the action of an organised ferment. He found that the *Mycoderma vini* was sometimes present, setting up acid fermentation. The bitter taste sometimes developed was also traced to

organised ferments, some of which were merely moulds or mildews. He found that by carefully decanting wines from the lees, and then heating them up to about 140° F. they would keep very well, an organic ferment being destroyed at this temperature, so that if the bottles were carefully corked no decomposition would take place in the wine, which would remain in a perfectly sound condition. This process of Pasteurisation, as it is called, has been adopted with success in the sterilisation of milk, and of other substances that would be spoiled by being subjected to the action of a higher temperature.

Pasteur's researches on the silk-worm disease commencing in 1865 were the first that can be said to have definitely and conclusively proved by rigid experiment the constant connection between a particulate and living organism, and a specific disease. Pasteur's previous investigations in the domain of specific fermentations, to each of which he assigned a specific ferment-inducing micro-organism, and his further experiments from which he concluded that not only must there be a living seed from which living organisms, however low in the scale, can spring, but each form of organism must have its ultimate origin in forms like itself, had prepared his mind for this investigation. Before he commenced his work on the silk-worm farms at Alais Pasteur had never even handled a silk-worm, but at the request of his old master, M. Dumas, he determined to do what he could to help what was fast becoming a crippled industry on account of the fearful ravages of *pébrine* amongst the silk-worms. He at once turned his attention to certain small corpuscles that had already been found in diseased silk-worms and moths and their eggs by Italian Naturalists. It had even been proposed that the eggs of the silk-worms should be examined under the microscope in order that only those in which these corpuscles were absent might be hatched, but nothing had come out of this suggestion. Pasteur went to work in his usual methodical but energetic fashion. First the presence of the corpuscles was demonstrated in the egg, the worm, the chrysalis, and the moth. It might sometimes be missed in the earlier

stages of development, but in the final form, the *imago* or moth stage, it could always, if present, be easily enough demonstrated. By a series of feeding experiments the passage of these vibratory corpuscles was traced along with food into the intestine of the worm, then into the surrounding tissues ; healthy worms were found to become diseased when they were invaded by these corpuscles. Should a chrysalis stage be reached, instead of a cocoon a mere mass of corpuscles might sometimes be found, whilst those that reached the moth stage were often crammed with corpuscles to such an extent that they never produced eggs at all ; on the other hand, some were still capable of producing eggs, but these eggs in turn, infected from the commencement, could only give rise to diseased worms. Year after year Pasteur, assisted by a band of ardent workers, returned to the attack during the hatching season. His wife and daughter for the nonce became silk-worm farmers ; forcing houses for artificial hatching were erected, and at last the "peppered" condition of the silk-worms (they are mottled by dark patches scattered over the body, hence the name *pébrine*) was traced to its true cause. It was demonstrated also that these corpuscles could find their way into worms through the wounds made in each other by means of the sharp hooks which are placed at the ends of their six fore feet, which, passing over the excreta-infected leaves, and then over the skins of their fellows, transfer the corpuscles that have been voided from the alimentary canal of an infected worm to wounds made in a healthy worm. Although drying renders these corpuscles innocuous, this must, under ordinary circumstances, be continued for several weeks, and the dry dust in the earlier stages of the process still remains capable, if blown about and carried to the mulberry leaves of unaffected farms, of spreading the disease to healthy stock.

What was to be done? The remedy was simple. As soon as the moths have paired they are set apart, each pair on a little piece of linen. As soon as the female has laid her eggs, she is pinned by the wings to the corner of the cloth where she is allowed to dry. When the leisure time of the silk-worm cultivator arrives in autumn and winter,

the dried moth is pounded in a mortar, and mixed with a few drops of water. A drop of this is examined under a microscope. If corpuscles are found, the cloth with its little mass of eggs, is, along with the contents of the mortar, thrown into the fire; if there are no corpuscles present, the eggs are kept to be hatched when the warm weather returns. In this way worms, cocoons and moths free from *pébrine* are obtained, and the disease is kept well under control.

A second disease, *flacherie*, which was found to attack weakly worms, was also traced to its source—the action of microbes taken in along with the food, and setting up a special series of fermentations in the alimentary canal.

Speaking of the precautions to be taken in the selection of silk-worms, Pasteur wrote: "If I were a cultivator of silk-worms, I would never hatch an egg produced from worms that I had not observed many times during the last days of their life, so as to make sure of their vigour at the moment when they spin their silk. If you use eggs laid by moths the worms of which have mounted the heather with agility, have shown no traces of *flacherie* between the fourth moulting and mounting time, and do not contain the least corpuscle of *pébrine*, then you will succeed in all your cultivations."

The silk industry which at one time appeared to be on the verge of extinction was restored to France, which in consequence is to-day millions richer. The prolonged strain and the work in the artificially heated cultivating rooms, appears to have told upon Pasteur's health, and in October, 1868, when still only forty-five, he broke down, being seized with an attack of paralysis. One side only was affected, but the effects remained to the day of his death. The next year, however, found him again at work at Alais, and then at the Villa Vincentina, near Trieste, on a silk farm owned by the Prince Imperial, at which for ten years the silk harvest had not brought enough to pay the cost of eggs. Under Pasteur's direction the cocoons produced "gave to the villa a net profit of 26,000,000 francs," or over £1,000,000 sterling! All opposition had speedily to give way before such a signal success, and Pasteur's method was finally adopted by the silk-worm farmers of all nationalities.

At the close of the Franco-Prussian war, during which, of course, all scientific work ceased, especially as far as Pasteur, who was an ardent patriot, was concerned, he was anxious to recommence his investigations. Again his thoughts reverted to the question of fermentation, especially in so far as it bore on an industry which he thought might be revived in his beloved country. The diseases of beer were, he pointed out, the result of the activity of lactic, butyric, and other ferments which found their way into the wort during the process of brewing, either accidentally or along with the yeast. In order to get rid of these he tried to obtain pure cultures of yeast. In this he succeeded, and so far improved the character of the ferment. Further, by applying his process of Pasteurisation—heating in bottle at a temperature sufficient to kill most non-spore-bearing organisms—he was able to prevent the development of most of the organisms accidentally admitted, with the result that he practically revolutionised the method of brewing beer, and from a mere rule of thumb process converted it into one strictly accurate and scientific. He ultimately found that when the beer is made with pure yeast and under proper precautions, no extraneous organisms find their way into it at any part of the process, and no Pasteurisation is necessary.

This study of beer, however, was merely an interlude, and Robert Boyle's prophecy that "he that understands the nature of ferments and fermentation shall probably be much better able than he that ignores them to give a fair account of divers phenomena of certain diseases (as well fevers as others), which will perhaps be never properly understood without an insight into the doctrine of fermentations," was now about to be fulfilled most completely and literally. Pasteur was not a physician or a surgeon, and it was but natural that the first practical application of his theories of fermentation to the study of disease in the human subject should be made by surgeons. In 1863 Traube in Germany pointed out that the alkaline fermentation of urine does not take place in the bladder except in those cases in which "vibrios find access to the bladder from without".

Pasteur had insisted upon this fact, and also pointed out that the fermentation was prevented by the presence of boracic acid. It was therefore proposed that in cases of operations on the bladder, the parts should be irrigated with a 3 or 4 per cent. solution of boracic acid. Mr. Guyon who had carried out this treatment at Pasteur's suggestion obtained marvellous results. In 1865, as already noted, Lister, then Professor of Surgery in the University of Glasgow, commenced his work on the antiseptic treatment of wounds. In 1874, five years after he had been called upon to occupy the Chair of Clinical Surgery in the University of Edinburgh, he wrote to Pasteur : " It gives me pleasure to think that you will read with some interest what I have written about an organism which you were the first to study in your memoir on lactic fermentation. I do not know whether you read the *British Medical Journal;* if so, you will from time to time have seen accounts of the antiseptic system which for the last nine years I have been trying to bring to perfection. Allow me to take this opportunity of sending you my most cordial thanks, for having, by your brilliant researches, demonstrated to me the truth of the germ theory of putrefaction, thus giving me the only principle which could lead to a happy end, the antiseptic system." Surgery was started on the path which has converted it from a mere handicraft to a noble art and a living science ; but medicine had not yet been established in the same way although numerous observers (among them, as Tyndall points out, Dr. William Budd) indicated that the specific epidemic and epizootic diseases were due to the action of seeds or germs which cannot originate spontaneously, but which must be "bred true" from pre-existing organisms which "are only propagated now by the law of continuous succession".

This brings us to the question of "anthrax," a blood disease, extremely fatal to animals, and often attacking the human subject. It is sometimes stated that Pasteur proved the causal nexus between the *bacillus anthracis* and anthrax, but although his work undoubtedly stimulated others to investigate the subject, and to investigate it successfully,

priority of actual demonstration cannot be claimed for him. His subsequent work on anthrax was, however, perhaps even more important than that which has been claimed for him.

The history of the discovery of the anthrax bacillus and its relation to anthrax may be told in a few words. It was first seen, in 1849, in the blood of an animal that had succumbed to an attack of splenic fever. In the following year Rayer and Davaine described it as occurring under similar conditions, whilst in 1857 Brauell noted its presence in the blood of a human patient suffering from anthrax. For six years nothing more was done, but in 1863 Davaine, stimulated by the study of Pasteur's observations on fermentation. returned to his investigations on anthrax, and continued working at the subject for ten years, with the result that he was able to give everything but actual proof that the bacillus anthracis was the exciting cause of this malignant disease. At this point Koch took up the work. He applied his plate cultivation method with solidifying media, and after working for some time announced in 1876 that he had been able to furnish most rigorous proof of Davaine's hypothesis. He isolated pure cultures of the organism, described its modes of development and reproduction, pointed out that under certain conditions, like some of the organisms already described by Pasteur, it formed spores, but that it also multiplied by a process of direct division or fission. During the following year Pasteur also succeeded in growing this organism as a saprophyte. His methods are always as ingenious as they are interesting. It had been found by Davaine that the anthrax bacillus might be demonstrated in a pure condition in the blood of living animals or immediately after death, but that eighteen or twenty-four hours after death the blood was invaded by a second organism, which, inoculated, produced symptoms that could not be ascribed to the bacillus anthracis. Pasteur was able to separate these organisms by making both anaerobic and aerobic cultures from the blood in which this mixture of organisms occurred. In the presence of air the anthrax bacillus only could grow, but in the culture from which air was excluded, only the septic vibrio could develop. The one organism

requires air for its existence, on the other air acts prejudicially.

The anthrax bacillus, he found, would grow readily in certain animals, the ox, the sheep, the rabbit, the guinea-pig, not so readily in the dog, whilst he found that fowls were not at all vulnerable to its attacks. What was the reason for this difference? The answer to this question, as worked out and given by Pasteur, so well illustrates his method of reasoning and procedure that it may be given in some detail. It was found that by subjecting the anthrax bacillus to a temperature of 44° C. (111·2° F.), it was rendered incapable of developing. The temperature of the fowl—41° to 42° C. (105·8° to 107·6° F.)—though not quite so high, may help the tissues to deal with the micro-organism and prevent its development taking place in the body of this bird. "No doubt," he said, "life to a parasite in the body of an animal would not be as easy as in a cultivating liquid contained in a glass vessel." Was this temperature, *plus* the action of the tissues, the cause of the non-development of the anthrax bacillus? The now classical experiment of cooling the blood of the hen—by placing its feet in water maintained at a temperature of 25° C. until it came down to the temperature of the human body—having previously inoculated the bird with the bacillus anthracis, was made with the result that "at the end of twenty-four hours the hen was dead, and all its blood was filled with splenic bacteria". The converse experiment was then tried. "A hen was inoculated, subjected like the first to the cold water treatment, and when it became evident that the fever was at its height it was taken out of the water, wrapped carefully in cotton wool, and placed in an oven at 35° C. Little by little its strength returned; it shook itself, settled itself again, and in a few hours was fully restored to health. The microbe had disappeared. Hens killed after having been thus saved, no longer showed the slightest trace of splenic organisms."

In these experiments and the deductions made therefrom, we have the groundwork of Pasteur's great work on protective vaccines and vaccination.

Following these experiments, or rather running concurrently with some of them, came the experiments with the microbes of fowl cholera which were destined to have such a tremendous influence on the development of latter-day medicine. In fowls suffering from fowl cholera there had already been described extremely minute micro-organisms which had been supposed by Moritz, by Peroncito, and also by Toussaint to be the cause of the disease. Pasteur isolated the organism in pure culture, grew it in alkaline chicken broth, and then by introducing small quantities of the broth culture (the one-hundredth or even the one-thousandth part of a drop) under the skin reproduced the disease in the fowl and the bird died. Pasteur found that by making successive cultures at short intervals, each new one from that made twenty-four hours before, the virulence of the bacillus of fowl cholera remained unchanged. If, however, a cultivation was kept for some time (three months) protected from dust, but with free access of air, it no longer retained its full virulence, and the birds inoculated with small quantities, though they suffered some illness, did not die. If, now, after they have passed through this mild attack of the disease, they are inoculated with the non-attenuated fowl cholera virus or cultivation—which before would undoubtedly have killed them—they may have a pretty smart attack of illness, but the majority of them recover. This was the first case in which an experimenter had been able to modify a virus outside the body, and so obtain a protecting virus or one capable of inducing a mild form of disease which would protect against the attacks of a more active virus. The difficulty of obtaining control over the virulence of a disease was overcome when the modified virus had been produced. Well may M. Valery-Radot quote M. Bouley, who said when Pasteur announced his work to the Academy of Sciences: "This is but the beginning. A new doctrine opens itself in medicine, and this doctrine appears to me powerful and luminous. A great future is preparing: I wait for it with the confidence of a believer and with the zeal of an enthusiast."

It was but the beginning; ere long Pasteur was able to

produce an anthrax virus so modified by heat that it would not form spores, after which by treating the micro-organism as he had already treated the fowl cholera bacillus he was able to obtain an attenuated virus which was capable of setting up a modified attack of the disease, and so of protecting the vaccinated animal against the attack of the more virulent microbe. His successful and striking experiments at Melun have been so often quoted that it is unnecessary to go into them again, but it may be well to point out that out of this work has arisen our knowledge that vaccination against anthrax, and against certain other diseases, can protect most efficiently in most cases, and for a considerable length of time; but it is evident that there are cases in which, either through the extreme susceptibility of the patient, or the intense activity of the infecting virus, only partial protection is obtained, whilst even in the most perfectly protected individual the effect of vaccination passes off after a time and leaves the patient again comparatively vulnerable to attack.

Having reduced the virulence of the fowl cholera and anthrax organisms, the next step was to see if their virulence could be restored. In the case of the bacillus anthracis this was done by passing the attenuated form through a very young guinea-pig, which, it was found, succumbed to the disease even when inoculated with the weak vaccine; a drop of blood from this animal inoculated into one a little older causes its death, and so on until the blood taken from an aged animal which has succumbed to the inoculation anthrax suffices to kill sheep, and that from the sheep, an ox. This method of exaltation of the virulence of a virus has since then been used with great effect in the production of active toxins necessary for the production of antitoxins, now so much spoken of. Before leaving this part of our subject, it may be well to point out that it was in connection with the fowl cholera group of organisms that Pasteur was enabled to show that the products of the micro-organisms, from which the micro-organisms themselves had been removed, were capable of setting up many of the symptoms of disease, that they had in fact active toxic or poisonous properties.

This was a most important step forward, especially as Salmon and Smith, two American investigators, were afterwards able to show that in the case of hog cholera the toxins derived from the specific bacillus associated with that disease were, when injected subcutaneously in minute doses or in an attenuated condition, capable of conferring an immunity against the attacks of the micro-organism. We have in these observations the foundation of the "antitoxin" treatment of diphtheria and tetanus, although the full details of the method could not at that time be worked out or even foreseen.

Working at the question of vaccination against swine fever, Pasteur found that when the swine fever organism is passed through several generations of pigeons it gradually becomes more virulent not only for the pigeon but also for the pig. When, on the other hand, the virus is inoculated from rabbit to rabbit, these animals die undoubtedly, but the blood inoculated into a pig does not set up an acute illness in that animal; it recovers and is proof against a fresh attack of swine fever. It must be noted however that, whether this organism is cultivated in the pigeon when it is most virulent, in the rabbit when it is least so, or in the pig when it is as it were in an intermediate condition, there comes a stage in the development of the organism, when, if kept to the one species, its virulence remains stationary—it is in fact a *virus fixé*, and will so remain until transferred to another species. This most important observation was destined to bear fruit in the great work on hydrophobia to which Pasteur devoted the last years of his life. Difficulty after difficulty was overcome. Inoculation with saliva failing, through the production of mixed infections, and Galtier having failed to produce rabies by the inoculation of blood from rabid animals, inoculation with cerebro-spinal fluid and portions of the central nervous system was tried; this time the inoculations were attended with success. Then instead of making his inoculation under the skin merely, Pasteur introduced particles of the brain or cord of an affected animal under the covering of the brain of a healthy animal, with the result that the disease most

certainly manifested itself in the inoculated animal, and at an earlier period than when the inoculation was made under the skin. Applying what had been found to occur in swine fever, Pasteur found that canine virus is more powerful than virus that has been passed through a long series of monkeys, whilst that passed through rabbits gradually acquires such activity that the period of incubation of the disease,—*i.e.*, the time which the disease takes to manifest itself after inoculation—is reduced to one half. Here then it was possible to obtain materials for inoculation of all degrees of virulence. Was it not possible by a process of gradual acclimatisation of the tissues to the presence of the virus to render a patient immune to the action of the most powerful virus? This was proved, in the first instance, by the use of monkey virus, and then, virus from dogs and rabbits, but all the more recent experiments have been carried on with material derived from the spinal cord of the rabbit. Having observed that the cords of rabbits that had been dead for some time contained a less virulent poison than the cords of freshly killed animals —this being especially the case in dry weather—he came to the conclusion that he might obtain vaccines of all degrees of virulence by simply exposing the cords of rabbits that had succumbed to hydrophobia to dry air for different periods. The fresh cord would give the strongest virus, that exposed longest (fifteen days) the weakest. This was found to be the case, and dogs were protected and *cured* even when the treatment was commenced only after the animal had been bitten by a rabid animal.

The application of this method to the human subject was first made in the case of a little lad, Joseph Meister, aged nine years, who had been badly bitten by a dog. As the dog was undoubtedly mad, and as there was little chance of the survival of the child in the ordinary course of events, Pasteur resolved after consulting with Professors Vulpian and Grancher, who agreed to share the responsibility, to treat the boy as he had treated the dogs that he had already been successful in protecting. The boy recovered, and since that time there can be no doubt that, although there

may have been certain failures, a great number of lives have been saved which otherwise would inevitably have been lost.

This investigation brought Pasteur's active experimental work to a conclusion, though he was in constant attendance at the laboratories in the Rue Dutôt or in those connected with the Stables at Garches, situated in the Park of the Old Château de Ville Neuve l'Etang, where the work on hydrophobia and diphtheria is carried on, guiding, advising, directing and watching with keen interest the work of his pupils, encouraging them to overcome difficulties and rejoicing with them in their successes. He thus practically died in harness, for it was at Garches after about three weeks' severe illness—he had been ailing for some time—that he died on the 29th of September.

Pasteur married the daughter of M. Laurent, the Rector of the Strasburg Academy. His son, Jean Baptiste, is in the Diplomatic Service, and his only surviving daughter, Madame Valery-Radot, is the wife of the biographer of the great man.

It is difficult to sum up in few words the character of such a man. Those who have come in contact with him were always struck by his energy, directness and simplicity; those who have followed his work, by his industry, his pertinacity, his clear-sightedness, his wonderful powers both of experiment and of deduction, and by that power of induction which amongst scientific men is found only in those who may lay claim to genius. When, on the occasion of the celebration of his seventieth birthday, Pasteur's admirers and disciples, collected from almost all civilised nations, met to do him honour, Sir Joseph Lister, presenting what might be called an International Gold Medal, spoke to the following effect, he was but giving utterance to what every man sitting in the Sorbonne felt most sincerely. He said: "There is certainly not in the whole world a single person to whom medical science is more indebted than to you. Your researches on fermentation have thrown a flood of light which has illuminated the gloomy shades of surgery, and changed the treatment of wounds from a matter of doubtful, and too often disastrous, empiricism into a scientific art, certain and

beneficent. Owing to you surgery has undergone a complete revolution. It has been stripped of its terrors, and its efficiency has been almost unlimitedly enlarged. But medicine owes as much to your profound and philosophic studies as surgery. You have raised the veil which had for centuries covered infectious diseases. You have discovered and proved their microbic nature; and thanks to your initiation, and in many cases to your own special labour, there are already a host of these destructive diseases of which we now completely know the causes. This knowledge has already perfected in a surprising fashion the diagnosis of certain plagues of the human race, and has marked out the course which must be followed in their prophylactic and curative treatment. Medicine and surgery are eager on this great occasion to offer you the profound homage of their admiration and their gratitude."

Whatever may be Pasteur's claim to honour, and how fully that claim is accepted Lister's words convey, it will in future perhaps, even more definitely than it does at present, undoubtedly rest upon the work which he has done in elucidating the questions connected with the ætiology and prophylaxis of specific infective disease. This one idea is the root from which his work branches out in all directions. One line of thought, work, and reasoning was followed throughout. Each step in this path was carefully probed and tested before any further advance was made, and once gained, was never left until it had been made thoroughly sound as a point from which the next step might be made. This it is that makes his work so reliable, and assures for it the immortality to which only truth can attain.

Pasteur was a patriot, and France is to-day poorer than when her great son was alive; but the loss that science of all nations has to bear is infinitely greater, not because his work will perish, for that can never come to pass, but because he can no longer work except through those who during his lifetime have been his pupils, friends, and assistants. They, however, have already given proof that we may not look in vain to them for the continuation of the noble work that their master commenced.

GERMAN SIMS WOODHEAD.

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WILLIAMSON'S RESEARCHES ON THE CARBONIFEROUS FLORA.

BY the death of Dr. William Crawford Williamson, on 23rd June of this year, English Science lost a fine naturalist of the good old school, who had escaped the narrow specialisation of the present period, and who has left his mark on every department of Natural History. The object of the present article, however, is not to give an obituary notice of Williamson, or to attempt any account of his scientific work as a whole; that has been undertaken elsewhere (by Solms-Laubach, Carruthers, Seward, Lester Ward and others). What is here aimed at is to give a brief statement of the chief results of his researches in the special department of Fossil Botany (the Carboniferous Flora) on which his scientific energies were mainly concentrated during the last twenty-five years of his life.

Williamson's work on fossil plants dates back to the very beginning of his career, for when scarcely twenty he made important contributions to Lindley and Hutton's *Fossil Flora of Great Britain* (36), 1833-37; at least thirty-two of the plates in this classical work were drawn by him, and in most of these cases the letter-press is wholly or in part from his hand. In a paper on the fossil remains of the Yorkshire coast (20), 1836, many vegetable remains are enumerated.

His first important original work on a palæobotanical subject belongs, however, to the year 1851, when he pub-

lished his memoir "On the Structure and Affinities of the Plants hitherto known as Sternbergiæ" (21). These curious fossils, in their typical form, are cylindrical bodies, marked with constrictions at very short intervals, so that the whole has been compared to a pile of coins. Williamson was the first to prove that these *Sternbergiæ*, or *Artisiæ* as they were also named, represent casts of the medullary cavity of a *Dadoxylon*, the constrictions marking the position of the diaphragms by which the fistular pith had been chambered. The paper is an admirable one, and the anatomical details of the tissues of the stem were worked out with great accuracy for that period. We now know that *Dadoxylon* was the stem of *Cordaites*, so we see that Williamson, in this early paper, made one of the first contributions towards building up the very complete knowledge which we now possess of this remarkable extinct family of Gymnosperms.

The characteristic work of Williamson, on which his reputation as a leader in Fossil Botany will rest, was that on the microscopic structure of Carboniferous plants. The paramount importance of such studies is, perhaps, hardly appreciated by systematic botanists, who, in dealing with recent plants, are accustomed to rely almost entirely on the more external morphology, and particularly on that of the reproductive organs. Evidence of this kind, however, is often wanting in the case of fossil plants, and when we are dealing with families so far back in the past, and so remote from any now living, as those of the Carboniferous Flora, it is obvious that the mere external characters of *vegetative* organs must be a very insufficient guide. Indeed even where the reproductive organs are preserved, little can be made of them, at any rate if they are Cryptogamic, without reference to their minute structure. Consequently it is hardly too much to say that, as regards the earlier formations, no knowledge of the Flora, which is of botanical value, can be acquired without the aid of histological characters, in addition to those of a more obvious kind.

So far as the Carboniferous strata are concerned there is happily no lack of magnificently preserved material for structural investigation, and it was by the thorough study

of such specimens from British sources that Williamson rendered his great services to Fossil Botany.

It is of interest to learn how these investigations began, and fortunately we are able to explain this in Williamson's own words from autobiographical notes which he left :—

"Early in the fifties when I was commencing in an unsystematic way to grind down fragments of various objects for microscopic investigation, I found in a drawer of my cabinet a portion of a Calamite that had been extracted from one of the ironstone nodules of the Coal-Measures. I was not at that time provided with a lathe or any other form of cutting or grinding machinery for preparing microscopic sections, yet the Calamite presented indications that some structure might be found in it. Chipping off with hammer and chisel such fragments as appeared suitable, I ground them down on a flagstone, and thus succeeded in obtaining nine curious sections showing the structure of a woody zone where it was in immediate contact with the medulla. Having then no intention of making any special use of these preparations, they were put away in a drawer of the cabinet and almost forgotten."

Our author then goes on to tell how he soon provided himself with some simple machinery for section-cutting, and narrates the lucky chance by which he became possessed of a wonderful cast of the interior of a Calamitean stem, which, in conjunction with the specimens showing structure, did much to elucidate the anatomy (see Williamson, 9, pl. 21, fig. 31 ; and 22). The publication of a figure of this cast in Lyell's *Manual of Geology*, 1855, led to a correspondence with the French palæontologist, Grand 'Eury. This again called Williamson's attention to his old Calamitean slides, which ultimately formed the basis of his paper of 1869, "On the Woody Zone of an Undescribed Form of Calamite" (22). This paper marks the beginning of his more mature work, and is in itself of great interest, for the form described is that remarkable one which he named *Calamopitys*, the nearest approach to a true *Calamodendron* which has been derived from the English Coal-Measures. All other

Calamitean stems so far discovered in this country, belong to the Arthropitoid type, in which the structure of the wood is less complex.

A passage from the autobiographical notes already mentioned refers to this time, and gives an interesting idea of the spirit in which the work was undertaken :—

"I then formed a resolution which has been of the greatest service to me. Remembering how I had been misled in my early researches amongst the Foraminifera, by the authority of Ehrenberg, I determined not to look up the writings of any other observers until I had studied every specimen in my cabinets, and arrived at my own conclusions as to what they taught. Having thus formed my own independent judgment, I then turned to the works of other observers in the same fields to learn in detail what their views were."

To this resolution not only the great merits, but also some of the defects of Williamson's work may be traced. All his writings bear the stamp of absolute originality, and are free from the wearisome load of literary recapitulation by which so many modern scientific treatises are burdened. On the other hand, no one man's work can form a sufficient basis for valid generalisation, and in some cases Williamson would doubtless have arrived sooner at true conclusions if he had allowed himself to be a little more influenced by the researches of others.

But no investigator, however independent his spirit, could succeed in making his mind an absolute *tabula rasa* to start with, and so we naturally find some traces of the influence of the great Brongniart in Williamson's earlier papers. In the memoir of 1869 (p. 173) he says: "The Calamites seem to me to constitute a well-defined link connecting the Exogens with the Acrogens". Brongniart, as is well known, had in his earlier works united all the Calamites in one group, but had afterwards separated those with exogenous (or, as we should now call it, secondary) growth under the name of *Calamodendron*, reserving the genus *Calamites* for forms supposed to be destitute of secondary tissues. The former he regarded as "Gymnospermous

Dicotyledons" on account of their anatomical similarity to that class, while the latter were left among Cryptogams.

Williamson, in the passage above cited, shows that though already beginning to disbelieve in Brongniart's two groups, he still attached great systematic importance to the presence of secondary tissues.

In the next year our author published a paper "On a New Form of Calamitean Strobilus" (23). This relates to an interesting cone, more fully described at a much later date (14), of the type subsequently placed by Weiss in his genus *Palæostachya* (37) (vol. ii., p. 161), in which the peltate scales arise in or near the axils of the bracts, instead of being inserted midway between their whorls as in *Calamostachys*. Williamson now (1870) arrived at the conclusion that "the Calamites constitute essentially *one* large group of plants". "Their stems were exogenous so far as the woody cylinder was concerned, and closely related to those of the *Dadoxylons*. But on the other hand their fructification was Cryptogamic." He inferred that the Calamites were "a generalised type, afterwards differentiated through the *Dadoxylons* and the oölitic *Equiseta*, into the modern types of Coniferæ and Equisetiform plants". The mere occurrence of secondary growth was no doubt an insufficient basis for such a conclusion, though, in the opinion of the present writer, the possibility of a relationship between Calamarieæ and primitive Coniferous Gymnosperms may again have to be considered.

It will be of interest to trace rapidly the further development of Williamson's work on the Calamites. The first memoir of the great series in the *Philosophical Transactions of the Royal Society* is entirely devoted to this group. The author states that "the fundamental aim of this memoir is to demonstrate the unity of type existing among the British Calamites". In other words he showed that all British Calamites, of which the structure can be ascertained, are alike in possessing secondary tissues, and consequently that Brongniart's distinction between Cryptogamic Calamites and Phanerogamic Calamodendra has no existence so far as our native Carboniferous Flora is concerned. In the light of

our present knowledge we can see the great merits of this memoir, though they were hardly appreciated at the time. Conclusive proof is there given of the truth of the view of Germar, Corda, and Dawes, that the ordinary form of preservation of a Calamite is nothing but a cast of the medullary cavity; the significance of the various markings on such casts was rightly explained. The main lines of the anatomy of both old and young stems were accurately laid down, and the existence of true medullary rays in the secondary wood demonstrated, while some evidence as to the mode of branching was already brought forward. This memoir undoubtedly laid a sound foundation for our knowledge of the structure of the Calamitean stem.

Of the later memoirs the most important for the vegetative structure of Calamites is part ix. (1877), which contains a splendid set of illustrations of the anatomy. A series of very young stems are figured and described, and their resemblance at this early stage to stems of recent *Equiseta* is recognised. This had become a point of great importance, for the demonstration that the supposed stems, as shown in ordinary sandstone specimens, were only casts of the hollow pith, had to a great extent cut away the old grounds for regarding the Calamites as Equisetaceous. Consequently accurate anatomical evidence on this point was much needed. The position and mode of attachment of the branches were also very finely elucidated in this memoir, though of course many important details were only made out at a later time. In this paper the genus *Astromyelon* was founded, not having been previously distinguished from *Calamites*. A later memoir (part xii., 1882) was largely devoted to *Astromyelon*, with which the smaller specimens, described by Hick and Cash (38) under the name of *Myriophylloides*, were identified. It was not till a later time that we learnt the true nature of *Astromyelon*, which, as Renault has conclusively proved, is made up of the roots of *Calamites* and its near allies (39). This result has since been amply confirmed by the study of the English specimens. Although Williamson at the time failed to detect this fact, he displayed great acumen in uniting (and, as it has proved, quite correctly) forms so different as

those grouped together under the name *Astromyelon*, in his part xii. We now know that these organs are all roots and rootlets of Calamites.

Passing on to Calamarian *fructifications* we find that memoir v. (1873) contains some additional illustrations of the cone now known as *Calamostachys Binneyana*, already described by Binney and Carruthers (41 and 42). These authors had already recognised its true affinities as a form of Calamarian fruit. Williamson was unfortunately misled by a mistake as to the anatomy of the axis, and for many years refused to allow that *Calamostachys* had anything to do with *Calamites*. Ultimately he corrected this error himself, but it affects all his work on the subject up to 1890. The point is this: our author thought that the axis of the strobilus in *Calamostachys* was traversed by a solid strand of wood, something like that of a *Sphenophyllum* or, to use his nomenclature, *Asterophyllites*. In reality there is no resemblance in the anatomy of the two genera, for *Calamostachys* has in fact a well-developed pith, not to mention other differences (15 and 29). This anatomical error logically led Williamson to the conclusion that *Calamostachys* had much closer affinities with *Asterophyllites* (which he then regarded as Lycopodiaceous) than with *Calamites*. In memoir v., however, some interesting additions were made to our knowledge of the fruit, the most important being the first observation of the young spores still enclosed in their mother-cells, the remains of which had been mistaken by a previous observer for elaters.

In 1879 (mem. x.) further valuable specimens of *Calamostachys* were recorded. In one of the sections figured (pl. 15, fig. 15) the carinal or fascicular canals, so characteristic of Equisetaceous plants, are distinctly shown in the axis of the strobilus, but their significance was overlooked.

In the next year (mem. xi., 1880) a most important discovery—that of a heterosporous *Calamostachys*—was recorded. This form was at first referred to the original species, *C. Binneyana*, though subsequently shown to be specifically distinct. The author said: "This discovery

only strengthens my old conviction that the true affinities of the strobilus are with the Lycopodiaceæ". Yet a figure in the same memoir shows in the most striking way the resemblance of the peltate scales to those of *Equisetum*.

In 1887 the memoir appeared "On the True Fructification of *Calamites*" (xiv.). The strobilus described here is the same as that recorded in 1870 (xxiii.), but in the meantime several fresh specimens had been discovered, and the structure worked out in the most painstaking manner. The importance of this work consists in the fact that a fructification with axillary sporangiophores (*Palæostachya* of Weiss) was proved to be borne on a peduncle with the exact structure of a Calamitean stem. Williamson regarded this as the only fructification hitherto discovered, which had any claim to be regarded as that of a Calamite.

In 1888 (mem. xv.) Williamson recognised for the first time, though in somewhat guarded language, the existence of a pith in the axis of *Calamostachys*, and so gave up the one basis for his old position. In the same memoir he gives the finest figure of the peltate scales which has ever been published, and points out that they "approach even more nearly than they were previously known to do, to those of the living Equisetums". At this time he said nothing more definite as to affinities, but in his Index, part i., 1891 (25*) *Calamostachys* found its proper place at last in the family Calamarieæ.

In a paper published at Manchester in 1886 (25), a useful and very clear comparative account of the anatomy of the three genera, *Calamites*, *Calamopitys*, and *Calamodendron*, is contained.

The present writer does not think it necessary to make any detailed reference to the later memoir, in which he himself co-operated (29). A statement of the present position of our knowledge of *Calamites* will be found in Mr. Seward's article (43). Enough has been said to show in how large a degree this knowledge has been acquired through Williamson's labours.

The Calamites are a group the affinities of which may

now be considered as fairly well established. This is not the case with the family of the Sphenophylleæ, which we will take next in our survey of Williamson's work. This group is entirely unrepresented in the recent Flora, and its existence has only been revealed by palæontological research.

Our knowledge of these remarkable plants is chiefly due to two men—Renault and Williamson.

In the fifth memoir of the *Phil. Trans.* Series (1873) the latter author gave an admirable account of the structure of two species of *Sphenophyllum*, which he at that time described under the name of *Asterophyllites*. The form of the leaves in Williamson's Oldham specimens did not agree with the definition of the genus *Sphenophyllum* as then understood. We now know that the foliar characters have a very wide range of variation in *Sphenophyllum*, and Williamson's specimens are now placed without hesitation in that genus. In memoir v. all the main features of the anatomy were already described; the secondary growth was at once rightly interpreted by Williamson, though its recognition as such was long evaded by his French fellow-worker, in consequence of the invincible prejudice entertained by Brongniart's pupil against the possibility of cambial growth in a Cryptogam.

As to one of the two species investigated by Williamson (that now known as *S. plurifoliatum*) there has never been any doubt. The other, however (*S. insigne*), includes some unusually large specimens, with a very root-like transverse section, and showing some slight differences from the typical forms. Several of the chief authorities, such as Renault, Schenk, and Solms-Laubach, refused to recognise these larger specimens as *Sphenophyllum* or anything of the kind, and supposed them to be roots of some unknown plant. It turns out, however, that Williamson was perfectly right. The discovery of *Sphenophyllum* leaves on specimens manifestly identical with the disputed forms has settled the question, and the most formidable of the former opponents, Solms-Laubach, has now announced his conversion to Williamson's opinion (45, p. 247). The correctness of our author's

identification depended partly upon his sound estimate of the anatomical characters involved, and partly on his fine series of specimens.

Our knowledge of the fructification of *Sphenophyllum* is primarily due to Williamson, without whose researches we should have no idea of its real structure. As early as 1871 he published a paper on "*Volkmannia Dawsoni*, an Undescribed Verticillate Strobilus from the Lower Coal-Measures of Lancashire" (24), and already expressed the opinion that "it is the fruit either of *Asterophyllites* or of *Sphenophyllum*" (p. 37). In memoir v. of the series in the *Philosophical Transactions*, a fuller description of the cone was given, in memoir xvii. (1890) the discovery of its vegetative stem was announced, and lastly, in the following year (mem. xvii.) a number of new specimens were described, and all essential points in their organisation explained. The strobilus consists of an axis bearing many whorls of coherent bracts, from the upper surface of which the numerous sporangiophores arise; each of the latter is of considerable length, and bears a single pendulous sporangium at its apex. Up to this time the fossil was placed in Binney's genus *Bowmanites*, but its affinity to *Sphenophyllum* was always recognised by its discoverer. The full description of the structure given by Williamson enabled Zeiller in 1892 (46) to identify the fructification of the well-known *Sphenophyllum cuneifolium* as being of essentially the same nature, and thus to determine for the first time the true character of the fruit in that genus.

The organisation of *Sphenophyllum* is now as thoroughly known as that of a genus of recent plants, and few fossils have such indisputable claims to autonomy. The idea which still sometimes finds expression, that *Sphenophyllum* may represent the foliage of a Calamite, is due to a confusion of thought. Certain forms of foliage in some species of *Sphenophyllum* may doubtless bear an external resemblance to some forms of Calamitean foliage, but the genus *Sphenophyllum* is characterised by its structure and fructification, and not merely by the form of the leaf.

In his later works Williamson placed *Sphenophyllum* in the family Calamarieæ, following Weiss in this, but he fully recognised that their systematic position is still an open question. A remarkable, newly discovered fructification, lately described by Solms-Laubach under the name of *Bowmanites Römeri*, appears likely to throw some further light on the affinities of the group (45).

The next family which we propose to consider in illustration of Williamson's work, is one of which he may fairly be called the discoverer. This is the group Lyginodendreæ. Although a few specimens had been previously observed, the investigation and interpretation of their structure has been practically in our author's own hands. The first full account of the genera *Lyginodendron* and *Heterangium* occupies the fourth memoir of the series in the *Philosophical Transactions*. At this time the main points of the anatomy of the stem in both genera were determined. Williamson, with his usual acumen in such matters, at once saw that the two plants were closely related, though there is a striking difference between them, *Lyginodendron*, at least in the normal form, having a large pith with a ring of distinct bundles around it, while in *Heterangium* the wood extends to the centre, and there is no pith at all. At that time Williamson already suspected that the leaves which he then called *Edraxylon* belonged to *Lyginodendron*, and "then," he said, "strange as it may appear, *Lyginodendron* will become an undoubted arborescent Fern". This conclusion may well have appeared strange, for the anatomy of the stem of *Lyginodendron* is much like that of a Cycad, especially as regards its secondary tissues.

Restorations of the stems of both genera are figured in the memoir of 1872. They are extraordinarily good, that of *Heterangium* especially being so exact that it could not be improved upon at the present day after all the additional work that has been done.

The foliage was fully described in 1874 (mem. vi.) under the name of *Rachiopteris aspera*, and shown to be of the *Sphenopteris* type. The proof that it really belonged to *Lyginodendron* was, however, reserved for a much later

memoir (xvii., 1890), and has been finally confirmed by still more recent researches.

In the meantime (in mem. vii., 1875, and mem. xiii., 1887) a beautifully preserved fossil had been described in great detail under the name of *Kaloxylon Hookeri*. It was then looked upon as a distinct plant, though its discoverer recognised the similarity of its tissues to those of *Lyginodendron*. More recent investigation has proved that *Kaloxylon* is nothing else but the root of *Lyginodendron* (31).

In 1887 a second species of *Heterangium*, remarkable for the extraordinary development of its secondary phloëm, and for the exceptional perfection with which its tissues are preserved, was described. In this memoir Williamson speaks of *Lyginodendron* and *Heterangium* as being "possibly the generalised ancestors of both Ferns and Cycads," a view which, in the light of our present more complete knowledge, appears to come near the truth.

The proof of the existence of this intermediate group was certainly a discovery of the first magnitude, the importance of which, though ignored in this country, was fully recognised on the Continent.

It cannot be denied that in this and one or two other instances the great merit of Williamson's work was somewhat obscured by the prominence which he gave to certain mistaken ideas as to development. He was always disposed to interpret specimens of the same plant which differed in size and structure, as being necessarily developmental stages of one and the same organ, whereas such differences are just as often due to the order of the branch or to the level at which the section happens to be taken. Consequently when he found some specimens of a stem with little or no pith, and others with a large one, he thought it necessary to explain how the former developed into the latter, and this led to his theory of the development of the pith from "primitive cells which lurked among the vessels," and to further hypotheses which appeared to other botanists to involve mechanical impossibilities. In the special case of *Lyginodendron* the matter was further complicated by the fact that roots were not always distinguished from small

stems. This questionable theory no doubt produced a certain prejudice in the minds of botanists against work which in all other respects was excellent.

Just before the close of his life, Williamson, with a rare openness of mind, convinced himself that these views were mistaken.

The fourth group which we propose to deal with is that of the Lycopodiaceous plants. It is more difficult here than in previous cases to give in a few words any idea of what was accomplished, for the forms investigated are numerous, and a general survey of the present state of our knowledge has still to be made. As is well known, Brongniart had taken the same course with this group as with the Calamarieæ. The specimens of *Lepidodendron* which he examined (*L. Harcourtii*) happened to be without secondary tissues, while his *Sigillaria* of course possessed them to a conspicuous extent. Brongniart saw the distinction quite clearly (for he was a good anatomist for that day as well as a great systematic botanist), and attached so much importance to it that he placed *Sigillaria* among Phanerogams, while leaving *Lepidodendron* in Lycopodiaceæ. Williamson in his memoir ii. (1871), the first which treats of these plants, at once attacked the position of the French palæontologist. He says: "I am now in a position to demonstrate the complete unity of the plants which M. Brongniart has separated so widely, and to show that the transition from one form to another is so gradual as to necessitate the inclusion of the entire series in the Lepidodendroid family". Ultimately he completely justified his contention, and, with perhaps one exception, brought round all his opponents to his opinion.

The earlier memoirs on Lycopodiaceæ have two drawbacks. First, that the author applied the terminology proper to Dicotyledons not only to the *secondary* tissues (where such an application is justifiable) but also to the primary structure, which in Lycopodiaceous plants is of course quite peculiar. Secondly, he attached too much systematic importance to the occurrence of secondary growth, and even proposed a classification of vascular Cryptogams based on

this character. Both these defects, however, are limited to the earliest papers of the series, while from first to last Williamson's account of the actual anatomical facts was in all essentials correct.

It would take too long to follow in any detail the progress of our author's work on the fossil Lycopodiaceæ. No less than eight of the Royal Society memoirs are devoted wholly or in part to these plants, besides which there are three other treatises, namely, an important controversial paper which appeared in the *Annales des Sciences Naturelles* for 1882 (28), the great monograph on *Stigmaria ficoides* (27), 1887, and the last paper he ever wrote, that on the mode of growth of the *Lepidodendra*, which only appeared this year (26). The unrivalled wealth of material in Williamson's possession, with which he was constantly supplied by a band of enthusiastic fellow-workers, enabled him to place our knowledge of the structure of Lepidodendreæ on an exceedingly broad basis. The anatomy of ten different forms is described in his memoirs, most of which are known by their leaves to be certainly true *Lepidodendra*, or to belong to the closely allied genus *Lepidophloios*. In six out of the ten the occurrence of secondary thickening of the vascular cylinder (in addition to an enormous development of periderm) has been proved. The only form which attains any considerable size without developing secondary wood is *L. Harcourtii*, the species which originally came into Brongniart's hands, and unfortunately led him to his hasty generalisation. It is a fact of special interest that Williamson's *L. fuliginosum*, a form so similar to *L. Harcourtii* that it was for many years described under the same name, develops at a rather late stage a well-marked secondary zone.

Specimens of *Sigillaria* showing structure are rare compared with those of *Lepidodendron;* Williamson, however, was able to show that there is no essential anatomical difference between Lepidodendroid and Sigillarian stems. This established a strong presumption that, as the *Lepidodendra* are undoubtedly Lycopodiaceous Cryptogams, the same holds good for *Sigillaria*. This presumption became

a certainty, when, in 1884, Zeiller discovered the macrospores of a *Sigillaria* (47). Renault appears to be the only authority who still believes in the existence of Phanerogamic *Sigillariæ*. He continues to regard the *Sigillariæ leiodermariæ* as Gymnosperms, while acknowledging the Cryptogamic nature of the subgenus *Rhytidolepis*, in which the existence of macrospores has been proved. He considers the whole group as a transitional one, but for this view, however interesting, there appears to be no evidence. All that we actually know tends to show that *Sigillaria*, as a whole, is a Lycopodiaceous genus with the closest affinities to *Lepidodendron*.

Among the fructifications of *Lepidodendron* described by Williamson several afford excellent examples of heterospory. The strobilus of *L. Spenceri* is especially remarkable, the spores (which from their size would appear to be macrospores) being most curiously winged. This plant presents several peculiarities, and it seems probable that, as its discoverer suggested, it may turn out to be generically distinct.

Some of the most important parts of Williamson's work on Lepidodendreæ relate to the forms known as *Halonia* and *Ulodendron*. He produced evidence which seems to leave no doubt that these stems, formerly regarded as belonging to distinct genera, are nothing but the fruit-bearing branches of *Lepidodendron* or *Lepidophloios*, the tubercles or scars marking the insertion of the fallen cones (see especially mem. xix.).

As regards the subterranean parts of the fossil Lycopods, Williamson did more than any other observer to make their structure known. His monograph on *Stigmaria ficoides* (27) has been regarded by competent critics as his best work. Concerning the actual structure little remains to be done, but the morphology of the organs in question is still disputed. That *Stigmaria* represents the underground portion of various species of *Sigillaria* and *Lepidodendron*, as first discovered by Binney and Richard Brown, may now be regarded as certain. Williamson always maintained that the stout main axes of *Stigmaria*, which branch dichotomously,

are the roots, and the appendages which they bear, the rootlets. This has been questioned by various authors, who interpret the main axis as a rhizome, and the appendages, or at least some of them, as modified leaves. The question is a difficult one, more especially when we consider that in recent Lycopods the differentiation between stem and root is scarcely so sharp as in other vascular plants. Although the principal Stigmarian axes differ very widely both in anatomy and morphology from any known roots, yet the resemblance of their appendages to the monarch roots of *Isoëtes* or *Selaginella* is certainly very striking, and it is by no means improbable that Williamson's interpretation of their homologies may turn out to be the right one.

In the long controversy as to the secondary formation of tissues by means of a cambium in Vascular Cryptogams, Williamson had most English and German palæobotanists on his side. The brunt of the battle, however, was borne by him, and to him the honours of victory are due. How complete this victory was may be shown by the opinions expressed by two leaders of the French school, both of whom were among his former opponents. Grand 'Eury expresses himself as follows (48, p. 195): "In any case the opinion that the *Sigillariæ* and *Calamodendra* are Gymnosperms sees from day to day the number of its adherents diminish. At the end of my studies in Le Gard I have been led to consider them as highly organised Cryptogams."

Zeiller, in a letter to Williamson, dated 24th March, 1895, says: "I now at last think the question as to placing the 'Calamodendreæ' among the Cryptogams definitely settled. . . . Your work (29) enables me at last to assert without any reserve the Cryptogamic nature of all this group, as regards which I had thought it right until now to mention the opinions of Brongniart and Renault, though strongly opposing them."

This relates to the Calamariæ; as regards the Sigillariæ Zeiller had convinced himself long before. An interesting incident of the controversy when at its height was the publication by Williamson and Hartog of a controversial memoir on *Sigillaria* and *Lepidodendron*, written in the French lan-

guage and published in the *Annales des Sciences Naturelles*, 1882 (28), "a bomb thrown into the enemy's camp," as Williamson was fond of calling it. This paper certainly produced its effect, though it was not till a little later that conviction was brought home to most of the opponents. Ultimately our author was fully justified in saying as he does in his autobiographical notes: "The contest, so far as this great question of the existence of an active cambium-zone producing secondary wood in the stems of Cryptogams is concerned, was settled for ever".

No one who has studied the anatomy of carboniferous plants at first hand can fail to be struck, not so much by the mere occurrence of secondary growth in these ancient Cryptogams as by the close resemblance of the process, in most cases, to that which goes on in normal Gymnosperms and Dicotyledons. This is a remarkable fact, for we know that secondary growth may also take place in totally different ways, as in certain Monocotyledons.

We may be allowed to quote a few words from Count Solms-Laubach's obituary notice of Williamson in *Nature* (32), which places the importance of his results in a clear light: "It was thus made evident by Williamson that cambial growth in thickness is a character which has appeared repeatedly in the most various families of the vegetable kingdom, and was by no means acquired for the first time by the Phanerogamic stock. This is a general botanical result of the greatest importance and the widest bearing. In this conclusion Palæontology has, for the first time, spoken the decisive word in a purely botanical question."

Williamson's last paper on the Lepidodendreæ (26) has a somewhat melancholy interest. It cost him a vast amount of labour, quite at the close of his life, when he should rather have been enjoying his well-earned rest. His toil, however, was not thrown away, for by means of it he at last convinced himself that his long-cherished views as to the growth of the medulla and primary wood were mistaken. After giving the full details of his elaborate numerations and measurements, he says (p. 59): "Anyhow, it is obvious that my original hypothesis of an enlargement of the

Primary Xylem proceeding from above downwards, is incapable of explaining the facts recorded in the preceding pages. Hence, that explanation appears more likely to be found in some modification of the views of Graf Solms." He goes on to point out, with perfect justice, that certain difficulties still remain.

It requires a very noble spirit of self-abnegation for a scientific man, at the age of seventy-eight, thus to sacrifice to the cause of truth opinions which he had upheld for so long.

We have only touched on certain parts of Williamson's work, and have left an immense number of his investigations altogether unnoticed. It seemed best however to make a selection, and to choose as illustrations the four great types of Vascular Cryptogams on which his most striking results were obtained.

Some apology is perhaps due for the very slight reference which has been made to the work of other palæobotanists. This article relates merely to Williamson's own researches, and it would have been impossible, without extending it beyond all reasonable limits, to include any adequate account of the share of other observers in building up the present fabric of our knowledge.

We cannot conclude this sketch of Williamson's work on carboniferous plants without some allusion to his remarkable powers as a draughtsman—powers which most of us must be content to admire without emulating. Almost all the illustrations to his numerous papers, and in fact all those which relate to anatomical structure, are by his own hand. It is hard to understand how so busy a man can have found the mere time for such minute and detailed labour, but the character of the work (to which full justice is not always done in the published reproductions) bears witness, not only to his marvellous industry, but to rare artistic skill.

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The above list includes all Williamson's more important memoirs on the Carboniferous plants. As regards other authors only a few of the works more directly referred to are cited.

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MINERAL TRANSFORMATIONS.

AMONG the most interesting problems which present themselves to the mineralogist are those relating to the "Life-history" of minerals, if such an expression may be used to include their mode of origin, the changes to which they are liable and their decay.

Few minerals are able to resist the transforming influences which operate in the crust of the earth; the changes of temperature and pressure, contact with air, water and the liquids and gases which they contain, together with the slow but continued action of permeating solutions and of electrolytic processes. Apart from the constituents of certain deep-seated and comparatively unaltered igneous rocks, there are few, if any, minerals which have not resulted from the change, decay, or destruction of pre-existing minerals, and, conversely, few exist which are not destined in due course to beget by their disintegration certain other minerals. The history of their transformations possesses, therefore, a double interest, both as indicating the probable nature and order of the chemical and physical processes to which they have been subjected, and as a clue to the nature and constitution of the minerals themselves. To geologists and mineralogists they are for these reasons equally attractive subjects for study.

The problems involved are beset with peculiar difficulties; the initial conditions under which most minerals were formed are hard to realise and impossible to reproduce; the processes to which they have been subjected have operated for such long periods that experimental evidence can rarely be used for comparison.

Again, though it may be certain that a given mineral is the result of alteration, the greatest uncertainty often prevails as to the nature of the original substance, and, further, in many instances where the nature of the initial mineral is beyond doubt, its decomposition products may appear to be ill-defined mixtures which it is hopeless to identify with any known species.

For these reasons the subject of mineral transformations has been an exceedingly fruitful one for speculation, with the inevitable result that what was originally a mere guess has often become stereotyped into a positive assertion, and is accepted without question or criticism.

In one class of mineral substances alone are we on fairly sure ground.

Possessing a certain individuality, a crystallised mineral may under some conditions, although partially changed, retain sufficient characteristics for its identification; in the remarkable class of products known as "pseudomorphs," the identification is often possible even when the original mineral has been entirely converted into something different.

A mineral which is stable under one set of conditions, is frequently unstable under others; when exposed to the action of air, water, or various solvents it may be converted into new substances, and if the action has been sufficiently gradual, the resulting product may preserve intact the crystalline form of the mineral from which it originated. In such a case there can be absolutely no doubt that it was produced by the alteration of that particular mineral, and although the mode of its production may be doubtful, its origin is certain.

Such perfect pseudomorphs, for example, as those found at Wunsiedel in Bavaria, which, while consisting of pure Steatite, present the characteristic and unmistakable form of Quartz, can obviously have resulted only from the alteration or destruction of Quartz crystals. Hence the special and unique interest of such "pseudomorphs," which are, in fact, the only safe guide to a study of mineral transformations.

There are, of course, two possibilities to be kept in view; such pseudomorphs may have been produced by the alteration of the parent mineral, or they may be mere casts of the same. In the latter case we can only conclude that the original crystal has been removed by the action of some solvent, and that its place has been filled by the deposition of new material. Such replacements need not necessarily throw any light upon the constitution either of the initial substance or of the resulting product. Many, possibly the

majority, of the known pseudomorphs have originated in this way.

But there are others which can only have been produced by the gradual alteration of one mineral into another. It may be, for example, that a crystal is found having the characteristic form of Garnet, which consists at one end of unaltered garnet substance, while at the other it has been wholly converted into Epidote. Such a pseudomorph possesses the greatest interest; it not only raises the inquiry what radicles have been removed, what radicles have been introduced during the change, and by what agencies the transformation has been effected; but further it cannot fail to provoke some theory of the composition both of the initial and final compounds.

The complication of pseudomorphous products is not so great as might be expected. That among the varied conditions under which they have originated the total number of known minerals should be so small is always surprising; but that the apparently obscure products and mixtures into which they decompose prove on careful study to be identical with one or other of these well-established compounds is perhaps still more remarkable. Much of the material of pseudomorphs was formerly referred dubiously to mixtures of certain ill-defined species—clays and other hydrated silicates for example; it was scarcely possible to determine their nature by analysis, for it was uncertain how many different minerals they contained.

But since the introduction of microscopical methods there is no longer any excuse for such an evasion; the optical characters of the transparent minerals have mostly been determined, and it is possible to identify them in the minutest crystals.

The use of the microscope has consequently corrected many mistakes which were made by the early mineralogists in their determinations of pseudomorphous materials, and has revealed many new transformations.

Although recent study has done much to clear up the doubts which formerly prevailed regarding the nature of the resulting mineral, it cannot be said that there is the

same certainty about the original minerals from which pseudomorphs were derived. In far too many instances the wildest and most diverse views have been held about the initial composition of the crystals, sometimes owing to the fact that very similar, though not the same, forms may be characteristic of several different minerals.

Pseudomorphs of lead carbonate having precisely the common form of barium sulphate have been found at several localities, and have generally been called pseudomorphs of Cerussite after Barytes; it is obviously far more reasonable to suppose that they were derived not from barium, but from lead sulphate which is isomorphous with Barytes, and therefore has almost the same form, and that the pseudomorphs are really after Anglesite. Again, the Serpentine found at Brewster in New York, which presents a most perfect cubic cleavage, was referred to various cubic minerals which bear absolutely no relation to Serpentine, until Tschermak made the extremely probable suggestion that they are due to the alteration of Periclase, the cubic oxide of magnesium. That Periclase is elsewhere only found in minute crystals is no argument against such an explanation. A similar objection was raised to the idea that the altered tetrahedral crystals found on the River Achtaragda in Siberia, and known as Achtaragdite, might have been derived from the rare mineral Helvite which they resemble in form; yet crystals of Helvite quite as large as any of the Achtaragdite crystals were subsequently found at two or more localities.

Again, carbonate of lime is sometimes found in acute pyramidal crystals, under such conditions that it might reasonably be supposed pseudomorphous after Celestine; until a recent date this seemed an unlikely explanation, for Celestine was not known to occur in such forms; yet there is clearly no reason why the mineral should not grow in pyramidal crystals, provided that they have angles characteristic of Celestine, and it has recently been found at several localities in acute forms very similar to those of the pseudomorphs.

These are only a few examples out of many which might

be cited to illustrate the doubt which often prevails as to the original source of pseudomorphs.

The process by which they have been produced is often even more uncertain ; and yet those who have attempted a systematic treatment of pseudomorphs have usually devoted their attention mainly to the processes by which they may have been formed, with the object of elucidating the problems of chemical geology.

The two best known and most exhaustive treatises on the subject are the elaborate work of Blum, *Die Pseudomorphosen des Mineralreichs*, 1843 (with four appendices, 1847-1879), and the first volume of Roth's *Allgemeine und Chemische Geologie*, 1879, in which all the previously described pseudomorphs are collected and classified.

Thus in the classification of Blum, they are considered according to the relation between the initial and final substance as due (1) to loss of constituents, as when Gypsum is converted into Anhydrite by dehydration ; (2) to gain of constituents, as when Anhydrite is converted into Gypsum by hydration ; (3) to exchange of constituents, as when barium carbonate is converted into sulphate ; and (4) to total replacement of one substance by another.

In the classification of Roth, a distinction is drawn between those which are due to the simple and direct action of air, water and carbon dioxide, and those which result from the action of solutions containing mineral matter.

In the present state of our knowledge there is here necessarily much that is mere speculation, since, even if the initial and final products are certain, the intermediate steps and processes are rarely known to us.

Taking these and similar difficulties into account, it is clearly necessary in any arguments based upon pseudomorphs to select only those cases in which there is absolutely no doubt as to the nature of the original mineral. A complete revision of the work of Blum and Roth is sorely needed, if only to correct downright errors and to weed the doubtful from the certain.

In no application of such arguments is this more necessary than in those speculations with which the present

article is more especially concerned, namely, the inferences drawn from pseudomorphs as to the chemical constitution of minerals.

It is a curious fact that, disregarding those which are obviously mere casts or moulds, and therefore represent no necessary chemical relations, the true alteration pseudomorphs (metasomatic products) in which one mineral has been gradually transformed into another are few and simple.

If well proved and indubitable cases alone are examined it will be found that any given mineral usually alters into only a few definite compounds, and that any given compound is found pseudomorphous only after a few definite minerals. It becomes, therefore, important to consider whether there be not some relationship between the initial and final product independently of the mode in which the pseudomorph has been produced, and whether there may not be some community of composition between those minerals which result in the same product.

Chemical analysis of the natural silicates explains, as a rule, nothing but their percentage composition, and any attempt to understand their constitution more fully is necessarily based upon highly speculative reasoning. For a long time the only clue available was that afforded by the mutual replacement of one radicle by another in different samples of the same mineral, or in closely allied minerals belonging to the same isomorphous group.

In a recent memoir upon the constitution of the Silicates (*Bulletin of the United States Geological Survey*, No. 125, 1895), F. W. Clarke points out that the mineralogist does not, like the organic chemist, deal with bodies of known molecular weight which can be measured by the density of a vapour or by cryoscopic methods; consequently the only means available are: (1) isomorphism, which indicates similarity of chemical structure; (2) dissimilarity of form with identity of composition, *e.g.*, in Andalusite and Kyanite, which must be taken to indicate isomerism; (3) pseudomorphs; (4) the artificial production of alterations in the laboratory; (5) the thermal decomposition of silicates; (6) their artificial synthesis.

In the alterations of minerals "atoms or groups of atoms can be split off from a molecule and replaced by others, and the information so gained bears directly upon the question of chemical structure".

At the present time such evidence is being actively sought among the various products derived from minerals, either by the processes of nature or by experiments in the laboratory.

The experimental inquiry is one from which much may be expected, but too little has yet been done in this direction. The suggestive experiments of Lemberg, Doelter and others, are of the greatest importance, but cannot be adequately summarised in the present article. But the natural pseudomorphs, if due care be taken, as indicated above, to select only those which are beyond doubt, supply examples which can never be equalled by artificial means.

In Clarke's elaborate memoir frequent use is made of this line of reasoning, and several other papers have been recently published in which the transformations disclosed by natural pseudomorphs are made the basis of a theory of chemical constitution, on the following principle: we may conceive, it is argued, that while a silicate undergoes change, certain radicles constitute a comparatively stable kernel which persists as such, and unites with the incoming radicles.

Among the most familiar materials of silicate pseudomorphs are Kaolin, Serpentine, and Talc: they may be taken as simple and typical examples affording arguments as to the structure of certain silicates from which they have been derived.

(1) Kaolin $H_4Al_2Si_2O_9$ (in which the water is all constitutional) results from the decomposition of Orthoclase $K\,Al\,Si_3O_8$.

Scharizer (*Zeitschr. f. Krystallographie*, xxii., p. 369, 1893), who assumes that in the natural metasomatic alteration products the liberated radicles free just sufficient valencies to saturate the water or hydroxyl introduced, supposes the change to be due to the entrance of four hydroxyl radicles which saturate four freed valencies of

aluminium. Kaolin then has the composition $(HO)_4Al_2Si_2O_5$ and Orthoclase is $(Si_2O_5)(K\,Si\,O_3)_2Al_2Si_2O_5$, and both may be regarded as salts of dimetasilicic acid $H_2Si_2O_5$.

Clarke, on the other hand, is concerned to prove that the natural silicates are all substitution derivatives of normal salts (mostly of orthosilicates); he therefore regards Orthoclase as having the composition $K_3Al_3(Si_3O_8)_3$, *i.e.*, as a normal salt of the trisilicic acid $H_4Si_3O_8$. This acid he considers as equivalent to orthosilicic acid $H_4Si\,O_4$, and the acid radicles as mutually replaceable. Such a supposition is conveniently invoked to account for the undoubted fact (in accordance with Tschermak's theory of the Felspars), that Albite $Na\,Al\,Si_3O_8$ and Anorthite $Ca\,Al_2(Si\,O_4)_2$ are isomorphous and mix in all proportions. The relationship between Orthoclase and Kaolin is then shown in Clarke's interpretation by attributing to the latter the formula $H_3Al_2(OH)(Si\,O_4)_2$. Kaolin is in some respects closely allied to the Micas, and Clarke compares its constitution with that of the Lime-Mica, Margarite, $(Ca\,H)(Al\,O)_3Al(O\,H)(Si\,O_4)_2$.

Groth, for the sake of simplicity, regards Kaolin as a dimetasilicate in which the two hydrogen atoms of the acid are replaced by two univalent radicles $Al(O\,H)_2$, and refers Serpentine, Talc, Picrosmine and Pyrophyllite to the same acid as allied minerals.

(2) Talc $H_2Mg_3Si_4O_{12}$ is a very common result of the decomposition of magnesian silicates.

The alteration of Enstatite $Mg\,Si\,O_3$ to Talc suggests to Scharizer that Enstatite has the composition $Mg_4(Si\,O_3)_4$, and Talc $H_2Mg_3(Si\,O_3)_4$, so that the change is simply a replacement of Mg by H_2 in a normal metasilicate. Clarke, reasoning from the fact that upon ignition one-fourth of the silica of Talc is liberated, prefers to regard this mineral as $Mg(Si\,O_4)Mg_2(Si_3O_8)H_2$ or $H_2(Si\,O_4)Mg_2(Si_3O_8)Mg$, *i.e.*, as a pseudometasilicate which is to be taken as a compound orthosilicate and trisilicate. Enstatite he would therefore regard as a similar compound orthosilicate and trisilicate $Mg_4(Si\,O_4)(Si_3O_8)$. Groth had previously suggested that Talc might be a basic dimetasilicate $Mg(Mg\,OH)_2(Si_2O_5)_2$, while Tschermak, in order to show

its relationship to the Micas, regarded the mineral as a molecular compound $Mg_6(Si\,O_4)_3 . H_4Si_5O_{12}$, the two components of which are according to his theory constituents of the Mica group.

(3) Serpentine $H_4Mg_3Si_2O_9$ is the almost universal result of the decomposition of Olivine $Mg_2Si\,O_4$.

Scharizer regards both these minerals as di-orthosilicates; Olivine is then $(Mg_2O)_2Mg_4(Si_2O_7)_2$ and Serpentine $H_2(Mg\,OH)_2Mg(Si_2O_7)$.

Clarke regards both minerals as orthosilicates: Olivine as $Mg_4(Si\,O_4)_2$ and Serpentine as $(Mg\,OH)H_3Mg_2(Si\,O_4)_2$. This formula for Serpentine was suggested by the results of Clarke and Schneider's experiments with dry hydrochloric acid upon the mineral; they found that only one of the three atoms of magnesium is readily extracted, while the remaining two offer greater resistance to the action of the gas.

Brauns (*Neues Jahrb. f. Mineralogie* (1), p. 205, 1894) has attributed this difference to the presence of water; this causes the gas at first to act energetically upon the mineral which is then protected by the chloride formed. Experiments made by Lindner have also failed to confirm the results of Clarke and Schneider.

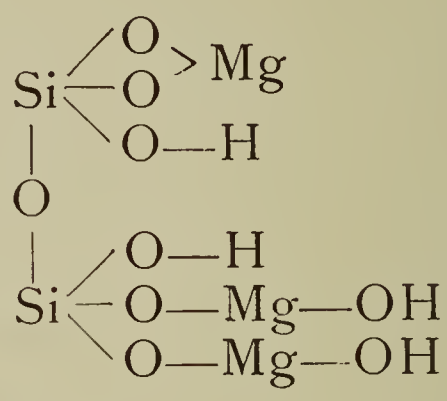
Brauns himself agrees with Scharizer in attributing to Serpentine the formula $H_2(Mg\,OH)_2MgSi_2O_7$, which can be derived by the removal of magnesium and accompanying hydration from $Mg_4(Si\,O_4)_2$ or Olivine, after passing through an intervening stage in which the composition is $H(Mg\,OH)\,Mg_2Si_2O_7$.

Two other facts must be borne in mind in devising a probable formula for Serpentine: firstly, that when it is heated the water is liberated at different temperatures, one molecule going at red-heat, the other at a higher temperature; secondly, that when Serpentine is fused it becomes converted into a mixture of Enstatite, $Mg\,Si\,O_3$, and Olivine, $Mg_2Si\,O_4$. Both these facts are more or less indicated by the formula just given.

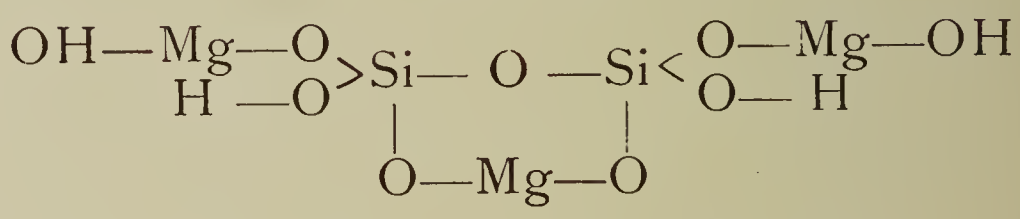
The graphic formulæ used by the above-mentioned authors in support of their various views have not been

reproduced in the preceding pages; the simplification, or complication, possible in such formulæ may be exemplified by the case of Serpentine; the structure may here be represented in three ways:—

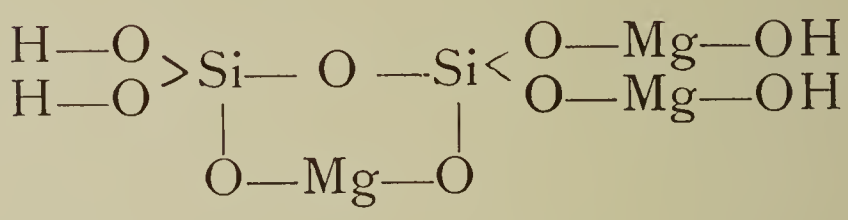
(i.)



(ii.)



(iii.)



The first is that adopted by Brauns, the second is that of Tschermak, but the third expresses perhaps even more suggestively the breaking up of the mineral into Enstatite and Olivine. Tschermak prefers the symmetrical grouping (ii.) as harmonising better with the derivation of Serpentine from two molecules of Olivine.

Groth suggests that Serpentine may be one of the two following molecular compounds, $H_2Mg_3(Si\,O_4)_2 . H_2O$ or $(Mg\,OH)_2Si_2O_5 . Mg\,(OH)_2$, the former representing a chloritic mineral with one molecule of water of crystallisation, the latter a molecular compound of Picrosmine and Brucite; but here we begin to lose sight of the relationship to Olivine which is suggested by the pseudomorphs.

I have entered at some length into the views which have been propounded concerning the chemical construction of Serpentine as an instance of the arguments used by these able chemists in their endeavours to solve the mystery of the natural silicates.

Such speculations should perhaps be regarded rather as illustrating the straits to which mineralogists are driven in their study of the silicates than as successful attacks upon

this difficult problem; they may excite the derision of organic chemists; but so long as the methods available in organic chemistry have no application in mineralogy, and until other means of exploring the constitution of even the simpler silicates are discovered, we have to make the most of the means at our disposal.

It will be observed that Serpentine and Kaolin have a precisely similar composition, three atoms of magnesium in the one being replaced by two atoms of aluminium in the other. Such common decomposition products deserve special study, since they undoubtedly represent simple substances of an extremely stable nature into which the less stable silicates of the igneous rocks tend to degenerate under ordinary conditions; no mineral is more stable than Talc; articles made of Steatite (a compact Talc), though it is so soft a material, have withstood the wear and tear of centuries far more successfully than most materials worked by man; it is difficult to believe that such minerals do not possess a very simple constitution.

For this reason also we might expect them to present some similarity not only among themselves, but also to those anhydrous minerals (pyroxenes, amphiboles, felspars) of which they are the hydrated representatives. It is, however, certain that before these relationships can be successfully traced, the natural pseudomorphs need most careful and detailed study in order that their initial and final states, and also the intermediate stages through which they pass, may be definitely established.

H. A. Miers.

ON SOME APPLICATIONS OF THE THEORY OF OSMOTIC PRESSURES TO PHYSIOLOGICAL PROBLEMS.

IN a large proportion of the most important functions of the animal body, and especially those classed under the heads of nutrition and secretion, the essential phenomenon consists in a transference of fluid, containing solids in a condition of solution or semi-solution, across a membrane of varying degrees of permeability. It is therefore natural that experiments by chemists on the phenomena of osmosis should have attracted the attention of physiologists and incited them to further investigations on the subject; and we find in the earlier text-books, of which I may especially instance the one by Ludwig, that the subject of osmosis occupies a large section of the work. Until recent years, however, our knowledge of the factors and forces involved in the interchange of substances in solution across animal membranes, was so meagre and inexact that the application of them to physiology could merely serve as a pretext for hazy speculation.

Our first exact knowledge of the forces concerned in osmotic phenomena was furnished by Pfeffer when he measured experimentally the osmotic pressure exerted by various solutions when enclosed in semi-permeable cells. The brilliant generalisations of Van t' Hoff have enabled us to form a correct estimate of the forces involved and of the work done in the osmotic transference of fluid from one side of a membrane to the other, although we do not yet know how it is that these solutions are able to attract water through thè cell-wall in order to exert an osmotic pressure equal to that which the molecules of the dissolved substance would exert if in the form of a gas. It was in 1877 that Pfeffer[1] and De Vries[2] published papers on the

[1] *Osmotische Untersuchungen.* Leipzig, 1877.

[2] *Die Mechanischen Ursachen der Zellstreckung.* Leipzig, 1877.

subject of osmotic pressures and cell turgor. Nine years elapsed before the subject was taken up by animal physiologists, and it is only during the last few years that we have awakened to the importance of Pfeffer and Van t' Hoff's researches and their manifold application to our science.

Hamburger[1] was the first, at the instigation of Donders, to apply these results to questions in animal physiology. In his first paper on the subject he confines himself to testing on blood-corpuscles the results obtained by De Vries with plant cells, and found that these corpuscles might be used for demonstrating the isotonicity of equivalent salt solutions. De Vries compared in different salt solutions the concentration at which the plasmolysis first began to take place. Hamburger found that red blood-corpuscles began to lose their hæmoglobin in salt solutions of a certain concentration varying with the nature of the salt. It was thus possible to determine the relative osmotic pressures of two salt solutions by observing how much distilled water must be added to each in order to produce breaking up of the corpuscles and tinging of the fluid by hæmoglobin. Hamburger in this way obtained results much exceeding in accuracy those got by experimenting on vegetable cells. The definiteness of the point at which the breaking up of red corpuscles begins is the more surprising, as Hamburger has shown that the membrane or limiting layer of the red corpuscles is not impermeable or "semi-permeable," in the sense that Traube's membranes are, but that it permits the passage of salts and even of proteids. The transfer, however, of substances from the corpuscles to the surrounding medium is invariably accompanied by a passage of other substances in the medium into the corpuscles, and this exchange always take place in isotonic proportions, so that the tonicity, or osmotic pressure, of the surrounding fluid, as of the red corpuscles, is unchanged. It would be difficult to give any physical explanation of this curious interchange, and Hamburger is evidently inclined to ascribe

[1] *Archiv f. Anat. u. Phys.*, p. 476, 1886, and p. 31, 1887, and other papers.

it to a vital action of the red blood-corpuscles, which merely means that the process is at present inexplicable.

Since the publication of the above paper, Hamburger has published a number of others on the relationships between the blood-corpuscles and plasma, and between the circulating blood and the extravascular lymph. To some of these we shall have occasion to refer later on.

In the meantime publications of Van t' Hoff and of Ostwald have given the impetus to a number of other researches by physiologists on the bearing of osmotic pressures on physiological processes. These researches fall naturally into two main groups.

In the first place, just as we can estimate the work done by a muscle by measuring the load raised and the height to which it is raised during a given time, so we can determine the minimum work done by a secreting cell by measuring the osmotic difference between the secreted fluid and the fluid from which it is formed, *i.e.*, the blood-plasma. Moreover, in many cases we can use a determination of the osmotic pressures in order to judge whether the absorption of a fluid across a given membrane is a simple physical process, dependent on osmotic differences, or whether the fluid moves against pressure, and the absorption must therefore involve the active intervention of the cells forming the membrane. Of works of this class, perhaps one of the most interesting is that by Dreser[1] on the work done in the secretion of urine.

Dreser makes use of the depression of freezing point to determine the osmotic pressures of blood or blood-plasma and urine. Since blood-corpuscles are destroyed in solutions of urea, they cannot be used as an index to the tonicity of this fluid. He points out in the first place that, from Van t' Hoff's results, it is possible to calculate directly the work done in concentrating a solution by the separation of a certain amount of pure water in the form of ice. If Δ is the depression of freezing point and T the absolute freezing point of the solvent (*i.e.*, for water, 273°,

[1] Ueb. Diurese und ihre Beeinflussung durch pharmakologische Mittel, *Arch. f. Exp. Path.*, xxix., p. 307, 1892.

and w the latent heat of fusion of ice = 79 cal.), then the work can be reckoned from the following formula :—

$$dA = \frac{\Delta w}{T} \times dv.$$

Thus for 1 per cent. solution of cane-sugar ($\Delta = \cdot 055$)

$$dA = \frac{\cdot 055 \cdot 79}{273} \times dv.$$

To reduce this result to gravitation units we must multiply by 424, and we thus find that to separate the volume dv of pure water as ice from 1 per cent. cane-sugar solution, a force is necessary equal to the pressure of a column of water of $\frac{\cdot 055 \times 79 \times 424}{273}$ metres in height.

A depression of $\Delta = -1^\circ$ corresponds therefore to an osmotic pressure of $\frac{79 \times 424}{273}$; that is to say, to 122·7 metres of water. We have therefore to multiply Δ by 122·7 in order to obtain the osmotic pressure in water of any solution.

We must now consider how these results can be applied to determine the amount of work done by the kidney cells in secreting a given specimen of urine. Dreser takes for an example the case in which during a night 200 c.c. urine were secreted with $\Delta = -2 \cdot 3^\circ$. This was separated by the kidneys from the blood with $\Delta = -0 \cdot 56^\circ$. We have to determine what work has been done in this process. This can be calculated from a simple formula. Let a be the amount of dissolved substance (a constant). This is dissolved in the variable quantity v, forming a solution containing y per cent. We thus get the equation $\frac{a}{v} = \frac{y}{100}$; the concentration y is, however, also a measure of the osmotic pressure. The amount of work necessary for pressing through a volume of water dv is therefore $dA = y \cdot dv$, or, since $y = \frac{100a}{v}$, $dA = \frac{100a}{v} dv$. Therefore $A = 100a \int \frac{dv}{v} = 100a \log_\epsilon v + C$. If the original quantity of the solvent was v_1, and the final amount v_2, $A = 100a \int_{v_2}^{v_1} \frac{dv}{v}$ or $A = 100a (\log_\epsilon v_1 - \log_\epsilon v_2)$.

This formula is similar to that for the work done in the compression of a gas at constant temperature. We can now apply this to the special case in which the kidney separated from the blood-plasma with $\Delta = -0{\cdot}56°C.$, 200 c.c. of urine with $\Delta = 2{\cdot}3°$.

In order to produce from 100 ccm. of a fluid with $\Delta = -0{\cdot}56°C.$, a fluid with $\Delta = -2{\cdot}3°C.$, the volume of the fluid must be reduced to 24·348 c.c. (0·56 : 24·348 :: 2·3 : 100). Hence 76·65 c.c. of pure water must be pressed out of every 100 c.c. of the original solution. By replacing these figures in the above formula we get :—

$$A = 100 \times 0{\cdot}56 (\log_e 100 - \log_e 24{\cdot}35).$$
$$= 100 \times 0{\cdot}56 (\log 100 - \log 24{\cdot}35) \times 2{\cdot}30258.$$
$$= 79{\cdot}11.$$

In this act of concentration, however, the 76·65 c.c. water which are pressed out enter a medium, not of pure water, but of lymph and blood with $\Delta = -0{\cdot}56°C.$ The osmotic difference therefore which has to be overcome in the concentration is not represented by 2·3, but only by 2·3 – 0·56°C. Hence the work done A will be not 79·11, but $79{\cdot}11 - 75{\cdot}65 \times 0{\cdot}56 = 36{\cdot}75$.

To reduce this result to gravitation units we must multiply by 122·7 ($\Delta = -1°C.$ corresponds to 122·7 metres water), and thus find that 4509·5 gram metres work are necessary to produce from 100 c.c. fluid with $\Delta = -{\cdot}56°C.$, 24·35 c.c. of secretion with $\Delta = -2{\cdot}3°C.$ Hence if the kidney has secreted 200 c.c. of urine with $\Delta = -2{\cdot}3°C.$, the work it has performed is represented by $\frac{200}{24{\cdot}35} \times 4{\cdot}5092$ kilogram metres = 37·037 kilogram metres.

Dreser then proceeds to investigate the work done in the secretion of concentrated and dilute urines, and also the effect of diuretics on this work. One interesting result he obtains relates to the maximum force of the kidney. From a cat which had been deprived of water for three days he drew off some urine with $\Delta = -4{\cdot}72°C.$ The blood at the same time had an osmotic pressure corresponding to $\Delta = -0{\cdot}66°C.$ These figures denote an osmotic difference of 498 metres water, *i.e.*, a pressure of 49,800 grams per

square centimetre. The absolute force of human muscle is 8000 grams, so we see that the mammalian kidney can exert a force six times greater than the maximum performance of voluntary muscle.

Of course these results absolutely disprove Ludwig's old mechanical theory of urinary secretion. According to this theory the urine is formed by a process of filtration from the glomerular blood-vessels. The fluid thus furnished is free from proteid and less concentrated than the blood-plasma. Since a blood pressure of 200 mm. Hg = 2·72 metres water, and $1^{\circ}\Delta$ = 122·7 metres water, the highest possible difference between dilute urine and blood could only be $\Delta = 0{\cdot}022^{\circ}$C. Dreser finds, however, that after beer drinking and in diabetes insipidus the urine secreted may have $\Delta = 0{\cdot}16^{\circ}$C., *i.e.*, a difference between Δ of blood and Δ of urine of ·4. Hence the production of urine by the glomeruli is also attended with the doing of work, and must therefore be looked upon as a process of secretion. Ludwig further considered that the dilute urine was concentrated on its passage through the tubules by the osmotic passage of water into the concentrated blood-plasma. This, however, is also impossible (as Hoppe Seyler showed long ago), since the urine as normally secreted is much more concentrated than the blood-plasma, and the difference may, as mentioned above, attain to enormous amounts (Δ of blood = $-\ 0{\cdot}56^{\circ}$C. and Δ of urine = $-\ 4{\cdot}7^{\circ}$C.).

In a paper published a few months ago,[1] Heidenhain applies the same method to deciding the question whether the absorption of salts and water from the intestines depends merely on the osmotic difference between intestinal contents and blood, or whether this act is carried out by the walls of the mucous membrane in opposition to the osmotic differences. In his experiments about 100 ccm. of salt solutions of varying concentration were introduced into a loop of the ileum a little above the iliacæcal valve, and the loop then returned to the abdomen. Half an hour

[1] "Neue Versuche über die Aufsaugung im Dünndarm." *Pflüger's Archiv*, lvi., p. 579.

later the abdomen was reopened, and the loop emptied of all remaining contents, which were analysed.

I will here quote one or two of the experiments as examples of the method employed and the results obtained.

(1) Dog 14·1 kilos. Blood serum : $\Delta = 0{\cdot}634$: NaCl = 0·620 per cent. Introduced 120 c.c. of salt solution containing 0·3 per cent. NaCl, and therefore ·36 grams NaCl ($\Delta = 0{\cdot}230$). Fifteen minutes later the loop only contained 18 c.c. fluid, with 6 per cent. NaCl (*i.e.*, 0·108 grams).

(2) Dog 7 kilos. Blood serum : $\Delta = {\cdot}642$.

(*a*) At 9h. 6m. injected 80 ccm. NaCl 1·98°/₀ = 1·58 gr. NaCl.
At 9h. 26m. found 80 ccm. ,, 1·20°/₀ = 0·96 ,, ,,
Absorbed in 20 mins. 0 ccm. ·62 gr. NaCl.

(*b*) At 9h. 30m. injected 80 ccm. NaCl 1·98°/₀ = 1·58 gr. NaCl.
At 10h. 10m. found 63 ccm. ,, 1·10°/₀ = 0·69 ,, ,,
Absorbed in 40 mins. 17 ccm. containing 0·89 gr. NaCl.

(*c*) At 10h. 13m. injected 80 ccm. NaCl 1·98°/₀ = 1·58 gr. NaCl.
At 11h. 13m found 42 ccm. ,, 0·96°/₀ = 0·40 ,, ,,
Absorbed in 60 mins. 38 ccm. containing 1·18 gr. NaCl.

The freezing points of the solutions were :—

Of the 1·98 per cent. solution	$\Delta = 1{\cdot}243$.
And of the unabsorbed fluid in (*a*)	$\Delta = 0{\cdot}916$.
(*b*)	$\Delta = 0{\cdot}802$.
(*c*)	$\Delta = 0{\cdot}790$.

It is evident, simply from these two experiments, that the absorption of water and salts from the intestines cannot be due to the osmotic differences between blood and intestinal contents.

In experiment (1) the osmotic differences would give rise to a transfer of water from intestines to blood, and of NaCl from blood to intestines. We find, however, that there is an absorption both of water and of salt.[1]

In experiment (2) the osmotic difference would occasion a flow of water from blood to intestines, and of salt from intestines into the blood stream. Here again, however,

[1] For a possible mechanical explanation of this result, see *Journ. of Phys.*, vol. xviii., p. 113.

the absorption of water from the intestines begins to take place long before the osmotic pressure of the fluid has reached that of the blood, and the lowering of the osmotic pressure takes place in consequence of absorption of salt, and not from a pouring out of water.

From a study of these and many similar experiments Heidenhain concludes that two kinds of processes are at work in intestinal absorption. In the first place, there is the physiological activity of the living cells, which take up probably a fluid of the composition of the intestinal contents; and, in the second place, there is the purely physical force of osmosis, which, according to the composition, may aid or subtract from the effect of the cellular activity. The result then is due to the algebraic summation of these two distinct processes, the one physical, the other physiological.

The physiological factor can be annulled by mixing with the salt solutions minute traces of a poison such as sodium fluoride. By this means the cells are paralysed, or hindered in their activities, and the absorption of water or salt now takes place in the direction determined by the osmotic differences. Magnesium sulphate appears to have a somewhat similar action, but to a much less marked degree.

Hamburger[1] has employed a somewhat similar argument to prove that lymph cannot be produced by a process of filtration, and that in its production work is done by the endothelial cells of the capillaries. I have pointed out elsewhere[2] several evident objections to his reasoning, and experiments of my own have shown me that the sooner the lymph is tested after it has left the vessels the closer is its osmotic pressure to that of the blood-plasma, and that the minute difference in osmotic pressures ordinarily observed between blood and lymph is due to the fact that this latter alters the tissue cells in its way to the lymphatics,

[1] "Untersuchungen über die Lymphbildung." *Zeitsch. f. Biologie*, p. 143, 1893.

[2] "The Influence of Mechanical Factors on Lymph Formation." *Journ. of Phys.*, xvi., p. 266.

and has its osmotic pressure raised by the waste products of the cellular metabolism.

These few examples will serve to show the many possible applications of the theory of osmotic pressures in determining the work done in the chemical functions of the body. It seems to me probable, however, that the theory may be of yet wider application. In the paper above quoted Heidenhain, after comparing the living cell to a steamer which may, and often does, float down with the stream, but can, under circumstances, move up against the stream, points out that our knowledge of the structure of this mechanism is so imperfect and restricted by instrumental shortcomings that physiologists cannot at present attempt to say how a given force is evolved in the cell, but must confine themselves to locating the exact seat of the energies in the body. I think this is perhaps rather too pessimistic a view to take of our possibilities. Secretion-pressure, movement, are among the phenomena regarded as essentially vital, and yet the botanist will explain root-pressure, movement, as consequences of changing osmotic pressures. We know already that every manifestation of activity is associated with katabolic changes, that is, a breaking down of large unstable molecules into smaller and more stable molecules, so that, wherever these changes are going on, there must be a rise of osmotic pressure. All we want is a suitable mechanism to direct the process, and it is possible that we may explain muscular work, secretion-pressure as directly due to these changes in osmotic pressure.

The possibility of such an explanation seems to have struck several observers, but, so far as I am aware, no serious attempt has yet been made to place an explanation along these lines on a firm experimental basis.

ERNEST STARLING.

THEORIES OF ELECTROLYSIS.

OUR knowledge of the phenomena which accompany the passage of currents of electricity through liquids has lately been much extended, and our theoretical ideas about the nature of electrolytes have undergone at least an equal development.

At the present moment it seems worth while to state the actual condition of the subject, and to trace the line between ascertained facts and the necessary consequences of those facts on the one hand, and the hypotheses which have been framed in order to explain them on the other.

The fundamental phenomenon from which we shall start is the truth of Ohm's law for electrolytes. This is an indirect deduction from the work which has been done by Kohlrausch and others on the measurement of the conductivity of liquids, and the accuracy with which the law holds has been investigated by Fitzgerald and Trouton and shown to be very great.

Considering the whole of a circuit containing an electrolytic cell, it is obvious that Ohm's law cannot express the facts, since no current will pass till the reverse electromotive force of polarisation is overcome. But polarisation only exists at the electrodes, and, once the current gets inside the liquid, its intensity is strictly proportional to the effective electromotive force which there acts. It follows that no chemical work can be done by the electrical forces acting within the liquid, so that either interchanges between the parts of the molecules must already be going on, or else the work done in tearing asunder one molecule must be given back by the formation of the next.

Since the former of these hypotheses is supported by the facts of chemical double decomposition, it is generally adopted, and we hence conclude that molecular interchanges are always going on, whether or not a current passes—the effect of the electromotive force being to carry a positively electrified stream of ions in one direction, and a

negatively electrified stream in the other. The parts which the molecules interchange with each other need not, as far as we have seen, remain dissociated for any appreciable time. The chemical properties of electrolytic solutions and their agreement with Ohm's law can be explained by supposing that interchanges between the ions only occur when two molecules collide. According to this view, the ions work their way through the solution by means of a continuous series of molecular decompositions and recompositions—the function of the electromotive force being to make the majority of kations pass to the next molecule on the kathode side, and the majority of anions pass to the next molecule on the anode side. An ion can thus only take a step forward when the molecule of which it forms part meets another molecule.

The most important contribution to the theory of electrolysis since the time of Faraday was made by F. Kohlrausch when he introduced the idea of ionic velocity.[1] He pointed out that the conductivity of a solution—that is, the quantity of electricity which passes in one second through a unit cube of the liquid when opposite faces of the cube are kept at unit difference of potential—must be proportional to the number of ions in unit volume multiplied by the velocity with which they travel past each other under the influence of the electromotive force. This relative velocity of the ions can, therefore, be calculated from measurements of the conductivity of an electrolytic solution. Now the ratio of the opposite ionic velocities can be found by observations on the unequal rate of dilution of the solution round the two electrodes when a current passes, as Hittorf pointed out, so that the absolute velocities of the ions, under unit potential gradient, can be calculated in centimetres per second.

Kohlrausch found that, in very dilute solutions, the conductivity was proportional to the concentration; the ionic velocities must, therefore, in such solutions, be constant.

[1] *Wied. Ann.*, 1879, 6, pp. 1, 145; 1885, 26, p. 161; 1893, 50, p. 385.

Moreover, he showed that the velocity of any one ion—sodium suppose—was independent of the nature of the solution containing it—the same, for example, in a dilute solution of sodium chloride as in one of sodium nitrate. In this way he was able to assign to many ions specific ionic velocities—numbers representing the speeds with which they move through a very dilute solution, when driven by a potential gradient of one volt per centimetre. These results were confirmed in two ways. Kohlrausch calculated the conductivities of various solutions from the velocities of their ions as found by observations on other solutions containing them, and showed that the values thus obtained agreed with those given by direct measurements, and Oliver Lodge and the present writer have directly observed the velocities of various ions, by tracking them through the liquid by means of indicators or changes in colour, and again confirmed Kohlrausch's numbers.

Passing to solutions of greater concentration, Kohlrausch showed that the conductivity increased more slowly than the concentration. If we call the number obtained by dividing the value of the conductivity by the concentration (expressed in gram-equivalents per litre) the molecular conductivity, we can express Kohlrausch's results by saying that, as the concentration of an electrolytic solution decreases, the molecular conductivity gradually rises and tends to a constant limiting value as the dilution becomes infinite.

The decrease in the molecular conductivity indicates a corresponding decrease in the velocities of the ions. It is much greater than can be explained by changes in the ionic viscosity, and must be due to some other cause. We must suppose that, in such solutions, the ions are electrolytically active only for a part of their time, and, during the rest of their existence, they are incapable of moving forward. We shall find other indications, both from chemical and from osmotic pressure phenomena, that, at any instant, a certain number of the molecules differs from the rest in condition, and the same number as we deduce from the conductivity is indicated by these two effects. Various hypotheses might be framed in order to explain the

physical difference between the active and inactive molecules. The most important of these is that due, in the first place, to Arrhenius, who supposes that an electrolytically active molecule is dissociated into its ions. We might, however, as far as we have yet gone, imagine that the necessary condition for electrolytic activity was a certain temporary arrangement of the atoms in the molecule, or that two molecules could interchange their ions only when they collided in a certain definite way. Let us, then, for the present, call the state of activity of a molecule *ionisation*, and not use the word *dissociation*, which commits us to one hypothesis.

It is evident from what has been said, that the ionisation of a solution of a certain strength (*i.e.*, the fractional number of its molecules electrolytically active at any moment) is measured by the ratio of its molecular conductivity to the molecular conductivity of a solution of the same substance at infinite dilution, when all the molecules are active. Thus, if we denote the molecular conductivity by the symbol μ, and its value at infinite dilution by μ_∞, we get for α, the coefficient of ionisation, the value μ/μ_∞.

In the year 1883, Arrhenius published a paper[1] in which he showed that this coefficient of ionisation was intimately connected with the chemical activity of the solution, and with the amount by which the osmotic pressure of an electrolytic solution exceeds that given by the solution of a non-conducting substance. The comparison has since been extended by Ostwald and others, and a few of the results are given below in a tabular form.

The subject of chemical affinity has lately been examined in the pages of this magazine by Dr. James Walker, and the idea of a specific coefficient of affinity, expressing a definite chemical property of any given solution, will be familiar to its readers.

In the following table, the first three columns give the constants of affinity of a few acids, measured in different ways: (I.) by observing the volume-changes accompany-

[1] *Mém. à l'Acad. des Sciences de Suède*, 1883. Also *B. A. Report*, p. 357, 1886.

ing actions between acids and bases; (II.) by allowing the acids to act on calcium oxalate, and determining the amount dissolved; (III.) by comparing the quickening influence of the acids on the decomposition of methyl acetate into alcohol and acid. Finally, in column IV. the relative electrical conductivities of normal solutions of the acids have been tabulated.

Acid.	I.	II.	III.	IV.
Hydrochloric	100	100	100	100
Nitric	102	110	92	99·6
Sulphuric	68	67	74	65·1
Formic	4·0	2·5	1·3	1·7
Acetic	1·2	1·0	0·3	0·4
Propionic	1·1		0·3	0·3
Monochloracetic	7·2	5·1	4·3	4·9
Dichloracetic	34	18	23	25·3
Trichloracetic	82	63	68·2	62·3
Malic	3·0	5·0	1·2	1·3
Tartaric	5·3	6·3	2·3	2·3
Succinic	0·1	0·2	0·5	0·6

The striking general agreement of these numbers is enough to show that an intimate relation exists between chemical activity and electrical conductivity.

It is found that the osmotic pressures of solutions of electrolytes are abnormally great when compared with those of solutions of non-conductors. By thermodynamic reasoning it can be shown that there must be a connection between the osmotic pressure and the depression of the freezing point, and the exact relation can be deduced. As more investigations have been made on the freezing points of solutions than on their osmotic pressures, it will be convenient to use the freezing points as the bases of comparison. In the case of solutions of non-electrolytes, it is found that the depression of the freezing point is the same for solutions of chemically equivalent strength, so that the effect must be proportional to the number of molecules, and independent of their nature. In solutions of electrolytes the depression of the freezing point is abnormally great, and it is found that the facts agree with the supposition that each electrolytically active molecule produces an osmotic pressure proportional to the

number of ions into which it can be resolved. Thus the effect of an active molecule of KCl is twice that of an inactive one, and the effect of a molecule of H_2SO_4 (which gives two H ions and one SO_4 ion) is, when in a state of ionisation, three times as great as that of a non-electrolyte. If, then, in a certain solution, we have m inactive and n active molecules, each of the latter giving k ions, the total osmotic pressure will be proportional to $m+kn$, whereas, if there were no ionisation, it would be $m+n$. Let us call the ratio of the actual to the normal osmotic pressure i, then

$$i = \frac{m + kn}{m + n} = \frac{m + n + kn - n}{m + n} = 1 + \frac{n}{m + n}(k - 1).$$

But $\frac{n}{m + n}$ is the fractional number of molecules which is at any moment ionised, *i.e.*, the coefficient of ionisation, α, or μ/μ_∞, so that it can be found by conductivity measurements, and we get

$$i = 1 + (k - 1)\alpha.$$

By placing the values of i thus calculated by the side of those directly observed from the freezing point, we get the following comparison :—[1]

Substance dissolved.	No. of gram-equivalents per litre.	i observed from freezing points.	i calculated from conductivities.	α coefficient of ionisation.
Methyl Alcohol -	0·1	0·97	1·00	0
	0·485	0·96		
	0·97	1·00		
Sodium Chloride -	0·117	1·93	1·84	·84
	0·194	1·87	1·82	·82
	0·539	1·85	1·74	·74
Potassium Sulphate -	0·091	2·35	2·33	·66
	0·455	2·04	2·06	·53
Acetic Acid - -	0·135	1·05	1·01	·01
	0·337	1·04	1·01	·01
	0·842	1·01	1·00	·00

Another method of tracing the connection between the two effects is to compare the coefficient of ionisation calcu-

[1] *Zeits. für physikal. Chemie*, ii., p. 491, 1887.

lated from the depression of the freezing point with its value as found from the conductivity. The following are some of the results given by H. C. Jones :—[1]

Substance.	Concentration in gram-molecules per litre.	Coefficient of ionisation.	
		Kohlrausch's result from conductivity.	Jones' result from freezing point.
Hydrochloric Acid -	0·002	1·00	0·984
	·01	0·989	·958
	·1	0·939	·886
Sulphuric Acid - -	0·003	0·898	0·860
	·005	·854	·838
	·05	·623	·607
Ammonia - - -	0·006	0·166	0·111
	·01	·130	·069
	·05	·061	·038

While the small differences probably indicate that secondary effects will ultimately have to be investigated, the striking general agreement shows that the condition of the molecules which makes them electrolytically active is also the chief cause of the increase in the osmotic pressure and of the existence of chemical activity.

So much seems to follow from the facts of the subject. We must now consider the hypotheses which have been framed in order to explain the physical meaning of ionisation. It is well known that the osmotic pressure of a substance in dilute solution not only obeys all the ordinary gaseous laws, but also has the same numerical value as the pressure which the same amount of substance would exert, in the same volume, were it to exist in the gaseous condition. Whether or not it follows from this that the mechanism of osmotic pressure, like that of gaseous pressure, is due to molecular impacts, is perhaps doubtful. It may be that finely divided matter must necessarily possess such properties, whatever be the ultimate physical explanation. Nevertheless, the agreement of dilute solutions with the

[1] *Zeits. fiir physikal. Chemie*, xii., p. 639, 1893.

gaseous laws shows that the osmotic pressure effects are proportional to the number, and independent of the nature, of the dissolved molecules. When, then, we find that solutions of electrolytes have abnormally great osmotic pressures of such a magnitude as to be proportional to the number of ions into which the electrolyte can be resolved, it is natural to test if the hypothesis that the substance is dissociated into its ions, in such a manner that the ions become physically independent molecules, cannot express the facts of the case.

That substances which show great chemical stability when solid are largely dissociated when dissolved is, at first sight, rather a startling idea. We must remember, however, that it is precisely those bodies which show the greatest amount of chemical activity in solution, that is to say, most readily exchange their ions with those of other molecules. The fact that a solution of potassium chloride does not show any of the properties of the elements, potassium and chlorine, though it has been urged as an objection, is not a conclusive argument against the theory of dissociation, for the ions are certainly under chemical and electrical conditions very different from those under which the elements exist in their usual forms. Another objection which has been brought forward is that the two ions would diffuse at different rates, and be therefore separable. But, since a definite charge of electricity is associated with each ion, it is obvious that, as soon as an ion got separated, it would leave the rest of the substance oppositely electrified, so that electric forces would be brought into play, tending to draw the ion back, and these would increase as more ions wandered away, till they prevented further diffusion. Still, some separation would occur, and a volume of water, in contact with the solution of an electrolyte, is found to take a potential of the same sign as that of the more diffusible ion, leaving the solution to assume a potential corresponding to that of the less diffusible ion.

Further evidence is given by the behaviour of semi-permeable membranes. A membrane of copper ferrocyanide can be prepared which will allow potassium chloride

in solution to pass through it, but is quite impermeable to barium chloride. Now, on the theory of free ions, some of the chlorine will still pass, since it could do so in the first case, but the electric forces will prevent any considerable separation from taking place. But, if we place some substance like copper nitrate on the other side of the membrane, the chlorine ions, which diffuse in one direction, are replaced by nitric acid ions, which diffuse in the other, and this process will continue till we soon find nitrate mixed with the barium chloride, and chloride mixed with the copper nitrate. The salts cannot have directly reacted with each other, for neither alone can pass through the membrane, but the phenomenon is readily explained on the hypothesis of free ions.

It has been shown by Ostwald and others that the properties of salt solutions (*i.e.*, of electrolytes) are largely additive, that is to say, can be found by superposing the properties of the ions which they contain. Such relations have been traced in the densities, colours, refraction coefficients, optical rotatory powers, surface tensions, viscosities, and thermal capacities.

Even stronger evidence, the weight of which seems not to have been fully recognised, is given by the results of Kohlrausch's measurements of conductivity. He proved that, in very dilute solution, the molecular conductivity was constant, that is, that the conductivity was proportional to the concentration. If we suppose that the ions are free from one another, and move forward under the influence of the electromotive force, this relation naturally follows. But if we reject this hypothesis, we have to suppose that an ion, which is then imagined to be free from an opposite ion only at the instants of collision, can take a step forward only when the molecule of which it forms part meets another molecule. Now the frequency with which such collisions occur is proportional to the square of the concentration, so that the average velocity with which an ion can work its way through the solution must also vary as the square of the concentration. But the conductivity is proportional to the number of ions multiplied by their average velocity, hence

the conductivity must, on this hypothesis, vary as the cube of the concentration.

The ordinary laws of chemical equilibrium have been applied to the case of the dissociation of a substance into its ions.[1] As this application is of fundamental importance, a short investigation must be here introduced. Let c be the number of molecules which dissociate per second when the number of undissociated molecules in unit volume is unity; then cp is the number when the concentration is p. Recombination can only occur when two ions meet, and, since the frequency with which this will happen is proportional to the square of the ionic concentration, we shall get for the number of molecules re-formed in one second

$$c'q^2,$$

where q is the number of dissociated molecules in one cubic centimetre. When there is equilibrium

$$cp = c'q^2,$$

If μ be the molecular conductivity, and μ_∞ its value at infinite dilution, the fractional number of molecules dissociated is μ/μ_∞, and the number undissociated $1 - \mu/\mu_\infty$, so that, if v is the volume of the solution containing one gram-molecule of the dissolved substance, we get

$$q = \frac{1}{v}\left(\frac{\mu}{\mu_\infty}\right) \text{ and } p = \frac{1}{v}\left(1 - \frac{\mu}{\mu_\infty}\right) \therefore \frac{c}{v}\left(1 - \frac{\mu}{\mu_\infty}\right) = \frac{c^1}{v^2} \cdot \frac{\mu^2}{\mu_\infty^2}$$

$$\therefore \frac{c}{c'} = \frac{\mu^2}{v\mu_\infty^2\left(1 - \frac{\mu}{\mu_\infty}\right)} = \frac{\mu^2}{v\mu_\infty(\mu_\infty - \mu)}$$

Let us put $\mu/\mu_\infty = a$; then a, which we have called the coefficient of ionisation, measures both the molecular conductivity referred to its limiting value as unity, and also the fractional number of molecules dissociated.

The equation then becomes

$$\frac{a^2}{v(1 - a)} = \frac{c}{c'} = \text{constant} = k.$$

Thus the value of this expression for solutions of binary electrolytes should be constant, and it has been confirmed by Ostwald and others for an enormous number of weak acids. In the case of solutions of strong acids and salt

[1] Ostwald's *Lehrbuch der Allg. Chemie.*

solutions, however, the value of k does not keep so satisfactorily constant, and no good explanation of the discrepancy has yet been given.

If we put a equal to $\frac{1}{2}$ in the equation, we find for k a value $1/2v$. Thus, $2k$ measures the concentration at which the electrolyte is just half dissociated.

Ostwald considers that this constant, k, gives the "long-sought numerical value of the chemical affinity," since it depends only on the nature of the substance.

In the case of substances like ammonia and acetic acid, where the dissociation is small, $1 - a$ is nearly equal to unity, and the equation becomes

$$\frac{a^2}{v} = k$$

or $$a = \sqrt{vk},$$

so that the molecular conductivity should be proportional to the square root of the dilution. This result has been fully confirmed by experiments on solutions of acetic acid.

The theory gives an immediate explanation of Hess' law of thermoneutrality, which expresses the fact that, in general, no heat change occurs when two neutral salt solutions are mixed. Since the salts, both before and after mixture, exist mainly as dissociated ions, it is obvious that large thermal effects can only appear when the state of dissociation of the products is very different from that of the reagents. Let us now consider the case of the neutralisation of a base by an acid in the light of the dissociation theory. In dilute solution, such substances as hydrochloric acid and potash are almost completely dissociated, so that, instead of representing the reaction as

$$HCl + KOH = KCl + H_2O,$$

we must write

$$H^+ + Cl^- + K^+ + OH^- = K^+ + Cl^- + H_2O.$$

The ions K and Cl suffer no change, but the hydrogen of the acid and the hydroxyl of the potash unite to form water, which is only very slightly dissociated owing to the fact that, being the solvent, it is present at a great concentration. The heat liberated, then, is almost exclusively that produced by the formation of water from its ions. An exactly similar

process occurs when any strongly dissociated acid acts on any strongly dissociated base, so that, in all such cases, the heat evolution should be approximately the same. This result is fully borne out by the experiments of Thomsen, who found that the heat of neutralisation of one gram-molecule of a strong base, by an equivalent quantity of a strong acid, was nearly constant, and equal to 13,700 or 13,800 calories.

In the case of weaker acids, the dissociation of which is less complete, divergences from this constant will occur, for some of their molecules have to be separated into their ions. For instance, sulphuric acid, which in the fairly strong solutions used by Thomsen is only about half dissociated, gives a higher value for the heat of neutralisation. Since the salts formed are almost completely dissociated, it follows that heat must be evolved when a molecule of sulphuric acid is dissociated into its ions. The heat of formation of a molecule from its ions is, of course, not the same as that associated with the formation of the molecule from its elements in the usual way, for the energy of the ion is very different from that of the atom in its normal state.

If we know the temperature coefficient of dissociation, it is possible to calculate the heat of formation of a gram-molecule from its ions by means of a thermodynamical relation, which was first applied by Clausius to the case of the evaporation of a liquid. In this way Arrhenius[1] has deduced the heats of formation of many bodies, from which the following are selected :—

	Heat of Formation.	
	At 21·5°.	At 35°.
Acetic Acid - -	+ 28	- 386
Propionic Acid - -	- 183	- 557
Phosphoric Acid -	- 2103	- 2458
Hydrochloric Acid -		- 1080
Soda - - -		- 1292
Potassium Chloride -		- 362
Sodium Butyrate -		+ 547

[1] *Zeits. für physikal. Chemie*, iv., p. 96, 1889 ; ix., p. 339, 1892.

By adding to these numbers for acids the heat of formation of water from its ions, Arrhenius has calculated the total heats of neutralisation of soda, and compared the results with the observed values for many acids.

	Calculated.	Observed.
Hydrochloric Acid -	13447	13740
Nitric ,, -	13550	13680
Acetic ,, -	13263	13400
Phosphoric ,, -	14959	14830
Hydrofluoric ,, -	16320	16270

The conductivity of a solution depends on two factors: (1) the dissociation, and (2) the frictional resistance offered by the solution to the passage of an ion through it. If we call the reciprocal of this resistance the ionic fluidity of the solution, the molecular conductivity will be proportional to the dissociation and to the ionic fluidity. At infinite dilution the dissociation is complete, and the ions are so far apart that no change in temperature can affect the state of dissociation. Any alteration in conductivity with change of temperature must then be due to an alteration in fluidity, and, therefore, the temperature coefficient of fluidity can be determined by measuring the temperature coefficient of conductivity at a dilution so great that the molecular conductivity has reached its limiting value. From the thermodynamical investigation it follows that, if the heat of formation is negative, that is, the heat of dissociation positive, the temperature coefficient of dissociation must be negative, so that the dissociation will decrease as the temperature rises. The increase in conductivity, shown by electrolytic solutions when they are heated, is, then, in general, due to the increase in ionic fluidity being greater that the decrease in dissociation. Now the table given above shows that the heats of dissociation are greater at 35° than they are at 21·5°, so that the negative temperature coefficient of dissociation must grow numerically greater as the temperature rises. If, at the same time, the positive temperature coefficient of ionic fluidity keeps constant, gets less, or in-

creases more slowly than the negative temperature coefficient of dissociation, it is evident that, at a certain temperature, the two will become equal, and that, beyond that temperature, any further heating will decrease the dissociation more than it increases the fluidity, so that the conductivity will again become less. The temperatures of maximum conductivity were calculated by Arrhenius for solutions of hypophosphoric and phosphoric acids, and shown to be 57° and 78° respectively. An experimental investigation actually gave maxima at 55° and 75°. More recently Sack [1] has found a maximum for copper sulphate solution at 96°. Calculation by Arrhenius' method gives 99° for a similar solution. Thus it appears that the thermal phenomena of electrolytic solutions are all satisfactorily explained by the dissociation theory.

An equally striking success has attended the application of the theory to the phenomena of the diffusion of dissolved electrolytes. A satisfactory account of the diffusion of non-conductors has been given by Nernst [2] and Planck.[3]

In any solution, the dissolved molecules are continually moving about in all directions. Therefore, in a given time, more molecules cross any fixed plane from the side on which the concentration is greater to that on which it is less than in the opposite direction. The more dilute parts of the liquid will thus grow stronger at the expense of those which were originally more concentrated, and this process will go on till the concentration is uniform throughout.

Now, the osmotic pressure of a solution depends on the concentration, and is approximately proportional to it, so that we may, for convenience, consider the variation in osmotic pressure, which exists in a solution of non-uniform concentration, to be the cause of the diffusion which goes on. Thus, if we put a layer of water over a solution of sugar, the osmotic pressure of the solution can be considered as driving the molecules of sugar into the water,

[1] *Wied. Ann.*, xliii., p. 212, 1891.

[2] *Zeits. für physikal. Chemie*, ii., p. 613, 1888; or, Nernst's *Theoretische Chemie.*

[3] *Wied. Ann.*, xl., p. 561, 1890.

and the driving force in any layer can be calculated from a knowledge of the variation in osmotic pressure gradient. If we replace the solution of sugar by one of hydrochloric acid, a further complication ensues. We know from Kohlrausch's work, that the velocity with which the hydrogen ions travel through a solution is, under an equal driving force, greater than the velocity of the chlorine ions, so that, at first, a separation goes on, positively charged hydrogen ions travelling quickly into the water, and negatively charged chlorine ions remaining behind. This at once explains the differences in potential observed when various solutions are put in contact with pure water. But, as this process of separation goes on, electrical forces, tending to prevent further separation, come into play between the oppositely charged ions, and presently a steady state is reached and the ions travel forward together. We can then equate the number of hydrogen ions crossing any layer in a given time under the influence of both the osmotic and the electric forces with the number of chlorine ions crossing the same layer, and from this calculate the actual rate of diffusion of the hydrochloric acid. The following table gives the observed and calculated values of the "diffusion constant," which may be defined as the number of gram-molecules of the dissolved substance which crosses unit area in one day when the osmotic pressure gradient is unity.

Substance.	D observed.	D calculated.
Hydrochloric acid - -	2·30	2·49
Nitric acid - - - -	2·22	2·27
Potash - - - - - -	1·85	2·10
Soda - - - - - -	1·40	1·45
Sodium chloride - - -	1·11	1·12
,, nitrate - - - -	1·03	1·06
,, formate - - - -	0·95	0·95
,, acetate - - - -	0·78	0·79
Ammonium chloride -	1·33	1·44
Potassium nitrate - -	1·30	1·38

In a similar way, the contact difference of potential between solutions of the same electrolyte at different con-

centrations or between solutions of two different electrolytes can be calculated. This is of great interest, for primary voltaic cells can be constructed, with a plate of the same metal for both electrodes, by placing the electrodes in solutions of different substances, or even in solutions of the same substance at different concentrations.

In the following table,[1] C_1 and C_2 denote the concentrations of the solutions round the opposite electrodes, and a comparison is made between the observed and the calculated values of the electromotive force.

Electrolyte.	C_1	C_2	E in volts (observed).	E in volts (calculated).
H Cl - - -	0·105	0·018	0·0710	0·0717
" - - -	·1	·01	·0926	·0939
H Br - - -	·126	·0132	·0932	·0917
K Cl - - -	·125	·0125	·0532	·0542
Na Cl - - -	·125	·0125	·0402	·0408
Li Cl - - -	·1	·01	·0354	·0336
NH_4Cl - - -	·1	·01	·0546	·0531
Na Br - - -	·125	·0125	·0417	·0404
Na $O_2C_2H_3$ - -	·125	·0125	·066	·0604
Na OH - - -	·235	·030	·0178	·0183
NH_4OH - - -	·305	·032	·024	·0188
K O H - - -	·1	·01	·0348	·0298

The difference of potential between metals and electrolytes is explained in a similar manner. Nernst supposes that each metal in contact with an electrolyte has a definite solution pressure, analogous to the vapour pressure of a liquid, in consequence of which ions are detached from it, and go into solution, carrying their charges with them, and leaving the metal oppositely electrified. The electromotive force of an ordinary voltaic cell will depend on the difference between the solution pressures of the two electrodes, and could be calculated from Nernst's theory if these pressures were known. When a current passes, the ions of the metal with the smaller solution pressure are forced out of solution by the others, and deposited on the electrode.

The electromotive force of the cell

[1] W. Nernst, *Zeits. für physikal. Chemie*, iv., p. 161, 1889.

Ag | 0·1 normal Ag No_3 | 1·0 normal K Cl with Ag Cl | Ag, in which silver electrodes are placed one in silver nitrate solution, and the other in silver chloride and potassium chloride, was calculated by Nernst to be 0·52 volt, and observed by Ostwald to be 0·51 volt.

The remarkable agreement shown between the results of experiment and the consequences deduced from the dissociation theory in the cases of the thermal phenomena and the laws of diffusion must be regarded as very strong evidence in favour of the substantial truth of that theory. No such agreement could arise unless there were at least a deep-seated analogy between the actual facts and the hypothesis on which the results have been worked out. It seems necessary to accept the view that the ions of good electrolytes are, in some way, free from each other for a considerable fractional part of their existence. This does not, however, exclude the possibility of the existence of molecular aggregates in solutions. Evidence of the existence of such aggregates has been collected by Pickering[1] and others, who have shown that, in varying the concentration of a solution, the physical properties often change at that point, at which its constituents are present in proportions corresponding to some simple molecular compound. Pickering made, for instance, a careful determination of the densities of solutions of sulphuric acid of different strengths, and drew a curve to show his results. Changes of curvature appeared at points corresponding to definite molecular proportions, and some of the hydrates thus indicated were subsequently isolated by crystallisation (*e.g.*, $H_2SO_4 . 4H_2O$, $HCl . 3H_2O$). These changes can more readily be seen if a new curve is drawn between the concentration and the rate of change of density with the concentration (*i.e.*, the tangents at different points of the first curve). By this process of "differentiation," a series of straight lines is obtained, with breaks at the positions where, in the first curve, changes of curvature appeared. Similar figures were drawn for the electric conductivity, expansion by heat, contraction on formation, heat

[1] Watts' *Dictionary of Chemistry*, Art. Solutions II.

of dissolution, heat capacity, refractive index, magnetic rotation, and freezing point, and changes of curvature were found at the same points for all. Even when the solutions become very dilute, similar changes of curvature occur, and, although it is impossible to say, in such cases, whether or not the points correspond to definite molecular proportions, it seems fair to conclude that the changes are due to the same cause as those which appear in stronger solutions. As we go on adding water to a solution of sulphuric acid, heat continues to be evolved, so that it seems necessary to suppose that one acid molecule is able to combine with, or, at all events, to influence in some way, an enormous number of water molecules, and this idea is confirmed by other facts. For instance, the volume of a substance in solution is in general smaller than its volume in the solid state, and, in some cases, even appears to be negative, thus showing that the water has been compressed. Again, the molecular heat (*i.e.*, the product of the specific heat and the molecular weight) of a solution is sometimes actually less than that of the water present, so that the thermal capacity of the whole, or, at all events, a large part, of the water must have been affected.

There seems, then, considerable evidence to show that hydrates exist in solution, or, at all events, that there is a tendency towards the formation of such definite molecular aggregates. It was soon noticed that it was in the case of electrolytes that there was the clearest evidence of chemical action between the solvent and the body dissolved. This suggested the idea that chemical action was the condition necessary for ionisation, and the formation of complex molecules from which individual ions could be easily removed by collision with other aggregates, the meaning of conductivity.

By approaching the subject from two different sides we have, then, arrived at two conclusions. Firstly, that pairs of opposite ions must be separated, and the ions exist free from each other's influence, and, secondly, that there must be a tendency towards the formation of more or less stable molecular aggregates between salt and solvent.

It seems, however, that these two conclusions need not be mutually inconsistent. Since it is evident that one salt molecule can influence a large number of water molecules, it follows that the chemical forces are very far-reaching. Each particle of salt extends its influence over a considerable region round it, and the properties of the solution will depend on its percentage composition. It is natural, then, that, when the composition of the liquid passes through the value which corresponds to a definite molecular compound, the fact should appear in some modification of the physical properties, although no definite aggregate moving about as an independent physical molecule need be formed. There is, therefore, no objection to the view that the ions exist free enough from each other to enable them to play the part assigned them by the dissociation theory.

It is, nevertheless, remarkable that bodies, so stable when solid, should be dissociated in solution, but a study of the properties of the solvent throws some light on the difficulty. If the forces between the parts of the salt molecule are electrical in their nature, as is quite probable, it follows that, as J. J. Thomson has pointed out,[1] immersion in a medium like water, of high dielectric constant, will cause these forces to become much weaker. The effect is best explained by imagining a large mass of conducting material to be placed near two small spheres charged with opposite kinds of electricity. Each charged sphere attracts electricity of the opposite kind to the neighbouring parts of the conducting mass, and to this induced charge most of the sphere's lines of force will then converge. Its influence on other outside objects will be, therefore, much lessened, and consequently the force of attraction between the two little spheres will be much reduced. The effect of an insulator of high dielectric constant is similar in kind though rather less in magnitude. This may explain the differences observed in the molecular conductivities of the same salt dissolved in different solvents, such as water and alcohol, for example, for, other conditions being the same, the effect

[1] *Phil. Mag.*, xxxvi., p. 320, 1893.

of solvents in loosening the connection between two ions, *i.e.*, their relative ionisation powers, will be proportional to their dielectric constants.

As well as loosening the connection between the two spheres, the conducting body will attract each sphere to itself. There will be, therefore, chemical forces between the free ions and the mass of the solvent. These forces will, however, be, in general, equal in all directions, so that mobility is secured, the ions being passed on, under the influence of the external electromotive force, from one portion of solvent to another.

Readers who wish to examine the subject more in detail may be referred to a book on *Solution and Electrolysis* by the present writer, and, for further developments of the dissociation theory, to Ostwald's *Lehrbuch der Allgemeinen Chemie*, and to Nernst's *Theoretische Chemie*. Useful summaries will also be found in Watts' *Dictionary of Chemistry*.

CECIL DAMPIER WHETHAM.

THE STUDY OF THE ANCIENT SEDIMENTS.

IN an article in the September number of "SCIENCE PROGRESS," attention was called to papers which appeared during the second half of the year 1894, which dealt with the Archæan and Lower Palæozoic Rocks; in the present paper it is proposed to consider writings treating of the Upper Palæozoic beds which were published during the same period. As usual the greater number of these are concerned with the coal-bearing deposits of various parts of the world.

In the September article certain papers were noticed which contained accounts of Upper as well as of Lower Palæozoic sediments, and it will be unnecessary to refer to these again, but before alluding to articles which are occupied with a description of beds belonging to a single geological system, I may allude to one paper containing notices of the various systems of the Upper Palæozoic rocks as developed in Central Asia (1). In the Southern Thian Shan, Middle Devonian rocks are found with the typical Mid-Devonian fossil *Stringocephalus Burtini*, and in the Central Kuen Lun is a Devonian *Stromatopora*-limestone. The Lower Carboniferous beds are also represented in those two regions, furnishing as they have done *Chonetes comoides* in the former, and *Streptorhynchus crenistria* in the latter region. The Upper Carboniferous (Moscow Stage) is found in W. Kuen Lun, as a *Fusulina*-limestone with *Spirifer mosquensis* and *Productus semireticulatus*. The uppermost Carboniferous strata are developed in S. Thian Shan, where they have yielded *Spirifer poststratus* and *Productus indicus*. In W. Kuen Lun are Permo-Carboniferous rocks with *Martinia planoconvexa;* whilst north of the Karakoram Pass are deposits which, formerly included in the Triassic, are now relegated to the Permian system.

It will be seen that our knowledge of the oceanic sediments formed contemporaneously with the abnormal Upper Carboniferous, Permo-Carboniferous and Permian deposits of Western Europe is rapidly increasing, and the time will soon come when the gaps in our information concerning the

interesting faunas connecting the Palæozoic and Mesozoic periods will be largely filled up; the supposed anomalies in the distribution of fossils which are still believed by many writers will then be satisfactorily explained.

There is little to add concerning the Devonian rocks; the only stratigraphical paper to notice is one treating of the rocks of the Altai Mountains (2). In this, Tschernyschew describes the fauna of the Krjukowsk-grube limestones, enumerating twenty species of fossils found therein, of which seven are new. The fauna of these limestones is comparable with that of the Coblenz group (Lower Devonian) of the Rhenish Provinces. The palæontology of the Devonian strata is enriched by another instalment of Traquair's *Monograph of the Fishes of the Old Red Sandstone of Britain* (3), in which the study of the *Asterolepidæ* is commenced, the genus *Asterolepis* itself being the subject of the contribution here noticed. The same author elsewhere (4) describes a new *Cephalaspis* from the Caithness Flagstones under the mame of *C. magnifica*.

Turning to the Carboniferous strata, we may notice in the first place a paper of considerable importance to the stratigraphical geologist, giving as it does the mature judgment of a palæobotanist on the value of fossil plants as indices of horizons in the Carboniferous rocks; I refer to the Vice-president's address to the Royal Physical Society of Edinburgh (5). In this address Kidston states his belief that plants are more useful than mollusca for separating the Carboniferous strata: for this purpose the writer uses assemblages of plants and not single species. He remarks that "many of the Calciferous sandstone species do not pass into the Carboniferous Limestone series, though they have some species in common; still the floras of the two periods are very different as a whole. Again, we find very few of the Lower Coal-Measure plants passing up into the Upper Coal-Measures, and even the few that do pass up are among the rarest of Upper Coal-Measure species, whereas they are the commonest species in the Lower Coal-Measures; even from the Middle Coal-Measures very few pass into the Upper

Coal-Measures. The Middle and Lower Coal-Measures have a much greater similarity in their flora, but here, too, we find each series characterised by its fossil plants." Appended to this valuable address is a table showing the vertical distribution of the Coal plants in Britain.

Referring to the author's view that the plants are more useful than the mollusca for separating the Carboniferous strata, it may be remarked that at the present time such is undoubtedly the case, at any rate so far as Britain is concerned, owing largely to the care which has been exercised in studying these plants by a number of palæobotanists, amongst whom the author of the address is prominent; but one cannot help thinking that the time is soon coming when the animal remains of the Carboniferous strata will be recognised by British geologists as being of far more importance in classifying those strata than they at present suppose. Reference has been made in an earlier article to Waagen's views on the value of fossil animals as an aid to the classification of the Carboniferous deposits, and the Continental geologists are far ahead of us in the use they have made of the invertebrata for this purpose, but British geologists are waking up to the feeling that there is much work to be done amongst the Carboniferous rocks of our islands, and it is to be hoped that the British Association will give its official recognition (if it has not already done so) to the desirability of doing further work amongst these strata, by appointing a committee to study the distribution of their invertebrate remains. It may be confidently predicted that, when this is done, very considerable changes will be introduced amongst the time-honoured lists of Carboniferous fossils found in our text-books.

Passing on to consider additions recently made to our knowledge of the British Carboniferous rocks, we may briefly allude to two papers written by Professor Boyd Dawkins. As these treat of the interesting subject of the extension of the Coal-Measures beneath the newer rocks of Britain, a subject to which much attention was paid at the recent meeting of the British Association at Ipswich, it will be desirable ere long to devote a separate article to this important question, and meanwhile we may

very briefly notice Professor Boyd Dawkins' papers written during the period under review. The first of these treats of the Isle of Man (6). A thickness of 1368 feet of deposit is developed near Peel; these deposits are referred to the Permian system, and compared with the sandstones and "brockrams" of Cumberland, which are known to be of Permian age. At Llen Moar, south-west of the Point of Ayre, after passing through 167 feet of drift, a limestone (referred to the Carboniferous Limestone) was pierced. To the north of this saliferous marls, placed by the writer in the Trias, occur. He concludes "that the Coal-Measures of Whitehaven do not range so far south as the Isle of Man. If they do occur, the only spot where they can be hoped for is in the extreme north of the island, to the north of the Carboniferous Limestone."

The second paper by this author to which we have to refer deals with the occurrence of Coal-Measures in the south-east of England (7). He accepts the views of the late R. A. C. Godwin-Austen, lately elaborated by Bertrand after a study of the folds of France and the adjoining regions, that the great tectonic folds affecting the older strata are traceable by the folds affecting the newer rocks on the surface, though the minor folds are not to be detected in this way. Applying these views to the south-east of England, he points out that the folds occurring amongst the older rocks of the south-west of England are traceable into similar folds affecting the rocks of the south-east of the island. Commencing with the southern folds and working northwards, we have the following alternation of anticlinal and synclinal curves.

(1) The syncline marked by the Culm-Measures of Devonshire, which is traceable in an eastward direction by the superposed Tertiary basin of Hampshire. The coal beneath this he considers to be probably of the Culm type and therefore valueless.

(2) The North-Devon anticline, traceable eastward into the anticline of the Mesozoic rocks of the Vale of Wardour and of the Wealden area.

(3) The Mid-Somerset syncline, probably represented in the Wealden area by the highly faulted strata north of the Rother.

(4) The Pembroke-Mendip anticline, continued eastward into the anticline of the Vale of Pewsey near Devizes, and past various inliers at Ham, Kingsclere, Peasmarsh near Guildford, Westerham Common, Charing near Maidstone, and so through the faulted area south of Ashford to the sea, south of Hythe. (Professor Prestwich takes this farther north to the sea, north of Dover.)

(5) The South Welsh syncline, corresponding with the London Basin amongst the newer rocks. According to Professor Dawkins this syncline probably widens eastwards. He considers that the Coal-Measures which were reached by piercing the newer rocks at Burford in Oxfordshire occur there owing to the existence of this syncline (Professor Prestwich expects coal to be found in isolated basins in this syncline in the Isle of Thanet, Essex, Hertfordshire and Oxfordshire). Our author places the Dover Coal-Field in the southern part of this syncline and recommends search to the west of Dover. He also advocates trial shafts to be sunk in various parts of the eastern extension of the South Welsh syncline, which he believes to be broken up into isolated basins (such as are found to the west where there is no covering of Mesozoic rocks, as, for instance, the Bristol and Somersetshire Coal-Field, and that of the Forest of Dean). The detection of these basins will be the result of experimental borings. The paper is illustrated by a map showing the presumed eastward extension of the various western folds enumerated above.

We have no paper definitely devoted to the study of the Carboniferous beds of the European Continent to notice. Of Asiatic Carboniferous (or newer) deposits we have several accounts in the volume of the *Records of the Geological Survey of India* published in the latter half of 1894. W. Saise (8) takes the Karharbari Lower Coal-Seam as the base of the Barakar beds (a grouping previously made by T. H. Hughes), and evidence is given against the inclusion of some of the Coal-Seams in the underlying Talchir beds as proposed by Feistmantel. There is no evidence of unconformity between the Talchir and Barakar beds, but the different facies of the two groups justify their

separation. In this coal field "the total thickness of the whole of the Talchirs when best developed . . . cannot be more than 300 feet. The greatest thickness of Boulder bed is 22 feet, and yet we meet these beds all over the field."

It will be remembered that Waagen places the Talchir stage as the lowest of his three Permo-Carboniferous stages succeeding the highest or Ardwick stage of the Carboniferous proper; the Karharbari is the Middle, and the Artinsk the Upper stage of this Permo-Carboniferous group.

Griesbach (9) records a small patch of Carboniferous rocks probably brought up by faults amidst the Mesozoic beds of the Chitichun area, north-east of the Uttadhum Pass. They have yielded many fossils including *Fenestella*, *Lithodendron*, brachiopods of the families *Terebratulidæ*, *Spiriferidæ*, and *Productidæ*, some cephalopods and trilobites.

P. N. Bose (10) describes the Moulmein group of the Tenasserim Valley. Here a limestone has furnished *Lonsdaleia*, *Lithostrotion*, *Productus*, etc., of Carboniferous age, and on the supposition that this limestone belongs to the Moulmein group, of which the writer has scarcely any doubt, that group is Carboniferous. It contains worthless coals (the younger workable coals of the area being probably tertiary).

T. D. La Touche (11) describes the Bhanganwalla Coal-Field of the Salt Range, and gives a map and section of the Coal-Field and adjoining district. Above the Salt-crystal pseudomorph group (of Cambrian age) is a Boulder bed succeeded by Coal-bearing beds. Above these are tertiary (nummulitic) beds.

There is one paper on the Carboniferous beds of Africa (12). Some time ago J. Walther announced the discovery of a Carboniferous Limestone in the Egypt-Arabian desert, which was referred by him to the Lower Carboniferous. In the light of further work the author refers these beds to the Upper Carboniferous, and we are thus furnished with a new area for the marine Upper Carboniferous strata. Amongst the fossils are *Fusulinella*, *Enteles ægyptiacus*, and other species, *Spirifer mosquensis*, *S. convolutus var.*, and *Dielasma hastatum*. The beds seem most nearly related to the Carnic *Fusulina* limestone and the corresponding Gshelian division of Russia (stage of *Chonetes uralica*).

In Australia we have a record of the discovery of Coal near Sydney (13). After passing through 2917 feet of the beds appertaining to the Hawkesbury series (Triassic), the bore came to :—

Permo-Carboniferous, Newcastle series,	Coal seam, 10 ft. 3 in. Clay, shale, and sandstone with *Vertebraria.*	1 ft. 9 in.

This genus *Vertebraria* is well known in the Permo-Carboniferous Damuda beds of India, with *Glossopteris.*

Professor David (14) discusses the position of the Australasian strata containing the *Glossopteris* flora about which so much discussion has arisen. He notes that it is the predominant plant, and is enormously abundant in the Permo-Carboniferous Coal-Measures of Queensland, New South Wales, and Tasmania. One doubtful locality (Arowa, N.S.W.) has been cited by M'Coy, where its age may be Carboniferous, but no well-established case has come under the author's notice where it has been found in Australia with *Lepidodendron* or *Rhacopteris.* In three doubtful cases of the occurrence of *Glossopteris* in association with Lower Mesozoic plants, the supposed *Glossopteris* may be *Sagenopteris.* In Queensland there is undoubted *Glossopteris* at a horizon which Jack considers Upper Cretaceous, but Norman-Taylor believes it to be Permo-Carboniferous, and the area where it occurs has not yet been mapped in detail.

Turning to the New World we meet with a paper by Professor Stevenson discussing a theoretical point. It is known that the volatile combustible matter in the Pennsylvania Coal decreases towards the east. The author's objections to hypotheses previously put forward to account for the change are given. He maintains that as regards its origin, the Anthracite of Pennsylvania does not differ from the bituminous coal of the Appalachian basin, "but because the great coal marsh from which sprang the many beds originated in the north-eastern corner of the basin, and extended thence on the advancing deltas formed by streams descending from the Appalachian highlands, the time during which the successive portions of the marsh would be exposed would be less and less as the distance from the north-east and northern border of the basin

increased, so that the extent of chemical change would decrease as the distance increased".

Several papers on Carboniferous Palæontology remain to be noticed. Treating of fossil plants, we find a paper by Kidston (16) in which a new genus *Plumatopteris* is described. It is a fern which finds its nearest allies in certain species of *Archæopteris*. Six new species of plant are also described in the paper. Count Solms-Laubach (17) also contributes to our knowledge of palæobotany. Turning to Palæozoology, we may refer to the publication of Brongniart's work on fossil insects of the Palæozoic rocks, though this was published in 1893 (18), but as this beautiful work has been noticed elsewhere, it will be sufficient to quote the title. Dr. H. Woodward (19) describes two new *Phillipsiæ* from the "shales with limestones" referred by Tiddeman to the upper part of the Mountain Limestone series from the banks of the river Hodder, occurring in the disturbed tract of Carboniferous strata lying south of the Craven Fault. Thanks to this author's work on the trilobites of the British Carboniferous strata, this group of organisms will probably be of considerable value in the immediate future in classifying the Carboniferous strata, as it has already been found in separating the various members of the Lower Palæozoic Strata.

The last work to notice is the commencement of a Monograph on *Carbonicola*, *Anthracomya*, and *Naiadites* by Dr. Wheelton Hind (20). Professor Hull has spoken of this group of shells as having been "a bane to palæontologists," and there has certainly been great confusion both with regard to the synonymy of the group, and the habitat of the forms belonging to it.

This confusion, thanks to the author of the Monograph under consideration, seems likely to be removed. He has "been reluctantly obliged to acknowledge that in two, at least, of the three genera to be described in this Monograph the names now in use must be abandoned for others which have the priority. M'Coy's name of *Carbonicola*, though accompanied by a partly erroneous diagnosis, is undoubtedly a few months older than King's *Anthracosia*, in which, too, the hinge characters described belong to a species and not

to the genus. And, also, there is no doubt that Professor J. W. Dawson's *Naiadites* is older than Salter's *Anthracoptera*, although Dawson's genus originally contained members of all the genera to be described in this work." Further information concerning the synonymy of the group may be obtained by reference to a paper by the present author published in the *Quarterly Journal of the Geological Society*, which contains a list drawn up by E. T. Newton, also in a paper by Professor Amalitsky in the *Quarterly Journal* for 1895, and in the report of the discussion thereon. With regard to the conditions under which the group lived, it is well known that Salter, in opposition to nearly all other authorities, upheld its marine character. The author presents and weighs all the available evidence bearing upon this question, and decides essentially in favour of a freshwater habitat. He sums up as follows: "On the whole very little positive evidence can be brought forward on behalf of a marine habitat for these shells, while there is, to say the least, a fair amount of presumption that they lived under freshwater conditions. It may be, however, that we have in the occurrence of *Carbonicola* and *Naiadites* with various marine forms at the base only of the Coal-Measures an example of a genus changing its habitat. It is probable that all molluscan forms were originally marine in habit, and that by modification and adaptation some few forms became able to exist in fresh water; and it is also probable that this adaptation took place at different times, and is even going on now, to wit, the discovery of *Hydrobia Jenkinsi* in inland canals of late years, which shell had till that time been only known under estuarine conditions." The author's view is thus substantially in accord with that previously expressed by Prestwich, John Young, Green, and others. The present contribution to this Monograph contains a description of the British species of the genus *Carbonicola* (*Anthracosia*).

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CARL LUDWIG.

THE development of the science of life has more than that of other knowledge gone forward in our days under the guidance and direction of just a *few* transcendent teachers. The scope and methods of modern physiology, in so far as it claims to be an exact and truth-loving science, lie confined within the limits which Johannes Müller and Carl Ludwig have set. We must go back to the Athenian days of Stoa and of Academy to find a personal influence as great as that exercised by the last great teacher. Life is for us the tritest, but at the same time the most marvellous, and the most awe-inspiring reality. Strange therefore that even in these days its fundamental doctrines should obtain but few masters for its exposition. In this respect we may perhaps detect a secret link between it and metaphysics, from whose speculative views physiology—a science of facts and substance—stands totally remote.

In Germany no single branch of natural science can boast a greater teacher than Carl Ludwig. Many are disposed to rank the scholar, the investigator above the teacher. A false conception! Who can seriously so underrate the latter, for he also "must by hard fighting have won conviction for himself in regions where no other could come to his aid, and must, in other words, have laboured on the confines of human knowledge, and conquered new dominion for it"?[1] A great teacher in the true sense is even superior to a great investigator; to the latter the meed of acknowledgment is accorded, if he has widened the limits at any special point of the vast territory; from the former however is demanded, that he be master of the whole, have an eye for every detail, and in wise foresight prepare for its fulfilment what the future has to ripen. The possession of these latter qualities by Ludwig is most conspicuous in his *Lehrbuch der Physiologie*, and through-

[1] Helmholtz, *Uber die akad. Freiheit u. s. n. Vorträge und Reden*, ii., p. 208.

out his activity as an academic teacher. What science owes to his own hands, and to his mind working through his numerous pupils, can be gathered from the annals of the laboratories of Marburg, Zürich, Vienna and Leipsic. Those who have not enjoyed his personal teaching are enabled by these to judge of the immense mass of new facts, the significant and ingenious method, and the extent of his investigations. These papers discover what a mighty plan lay at the root of Ludwig's labours, and with what firm perseverance he unwearyingly tried from all sides to decipher the problems of life. A number of men, well known to the science, with noble subordination ranked themselves as combatants in his general host, beside many simple comrades, who steadfastly followed the gifted leader; these shared his struggles and most toilsome victories. But it is from Ludwig's *Lehrbuch* and from his lectures that one can gain the deepest insight into the activities of his penetrating mind. In these the master ever held in prospect the whole territory, and the attentive follower came to understand *how* physiology is to be studied.

A thinker of genius, a philosopher (Arthur Schopenhauer) has declared that not the *Why*, but the *How* of things is the real task of knowledge. A modest, also a profound, judgment from the representative of a science in which the mind can unrestrainedly expand its wings, and in phantasy may order and measure things and ideas as its imagination may desire. Ludwig's greatest merit lies in the fact that he would not have the science of life treated otherwise than from the point of view of a study modestly confined to the nexus of natural laws governing the workings of the animal body.

The oft-quoted sentence introductory to his text-book: "Scientific physiology has for its task the establishment of the functions of the animal body, and the tracing of them consequentially from the elementary conditions of it," [1] expresses this with absolute clearness. His whole life work

[1] Die wissenschaftliche Physiologie hat die Aufgabe die Leistungen des Thierleibes festzustellen und sie aus den elementaren Bedingungen desselber mit Notwendigkeit herzuleiter.

has its source and spring in this principle. It will therefore hardly be deemed mere chance that he surpassed as a deviser of ingenious methods. What Kirchhoff did for mechanics Ludwig did for physiology. Unlike as the two men were in the outward form of expression, an unlikeness made still greater by the difference of the problems to which the two men devoted themselves, yet that difference between them was largely superficial, for they were so essentially alike in their conception as to how an exact science shall be treated. The sensation caused by Kirchhoff's introduction to mechanics is well-known history ; and how foreign and secretly unsympathetic the task appeared to many minds —to describe, perfectly and in the simplest manner, the movements occurring in nature. To have introduced this same conception into physiology long before Kirchhoff published his celebrated work—or successfully worked it out—is Ludwig's significant achievement. The daring of it was the daring of genius, and formed a link of close though deep relationship between the two contemporary investigators. Physiology owes to Ludwig no small number of discoveries of the highest importance; yet as a discoverer Ludwig's position is not unique ; our century has been a most splendid era for discoveries in physiology. It is the method of investigation, the manner in which the complicated phenomena of animal life were treated by Ludwig and his school that struck so brilliant light in the dim field previously covered by a cloud of speculations.

Ludwig like many other great savants had as a writer much of the artist in him. The addresses and the more popular lectures which he delivered occasionally are real masterpieces of composition and poetic imagery. This same gift of poetic imagination, this same remarkable talent for stating facts in a figurative and symbolic manner, guided his pen, when he—as he very often did himself—wrote the papers embodying the results of the researches of his pupils. The real attitude of Ludwig's mind as an investigator, the sober and matter-of-fact spirit in which he grasped the problems of animal life, has therefore sometimes been obscured by his artistic nature. Ludwig treated physiology

in his lectures in a manner quite different to that usual with many lecturers on natural science. He contented himself with drawing a simple logically-constructed sketch of the known facts. He who in some of his publications and in private conversation betrayed a charmingly artistic imagination, and sometimes an almost mystic foresight, showed in his lectures almost bald conciseness, and a pitiless logic. A great number of ideas, to which some teachers attach deep meaning, he ruthlessly treated as mere definitions, indispensable as symbols for the shorthand description of facts. It was with him a matter of conscience to impress on his pupils that inductive philosophy building upon correct observation forms the only basis for understanding the functions of the organism. Being well aware of the seriousness of the task of educating young men, who one day would be called upon to alleviate the agony of human suffering, to face with a firm eye a state of things against which speculative reasoning is an impotent weapon, he gave them for equipment the most precious instruments which he himself possessed: his conceptions of the how of life together with the best methods for studying it objectively.

It is no unnecessary digression to dwell here on the personal characteristics of the great physiologist. It was his personality that made him so great a teacher, and it was as teacher that he exerted indubitably an unparalleled influence over contemporary physiology. Nearly 300 pupils have, under his guidance, contributed by research to the furtherance of biology. Especially intimate were the relations between the Leipsic laboratory and British and American physiologists. Germany, Austria, Italy, and Scandinavia form a strong contingent of Ludwig's pupils, many of whom rank as the foremost physiologists in their respective countries, and, as his true heirs, work on in the spirit of his school. The school of Russian physiology owes its being almost entirely to Ludwig. Although, as already stated, Ludwig's power of attraction as a teacher was largely due to his personality, deeper reasons further contributed. Ludwig devoted his attention to

nearly the whole range of physiology, and not solely to a few special branches, so that, however varied the inclinations or the talents of the pupils were, he was able to allot to each a suitable task. The single field that lies untouched by him is, perhaps, the physiology of the senses; von Kries in his memorial of his late master has explained the wherefore of this, namely, that Ludwig entertained mistrust of the subjective methods indispensable to this kind of investigation. As a natural consequence of the wide field of work occupied in Ludwig's Institute, all the collateral sciences were enlisted to aid, macro- and microscopic anatomy, physics, and chemistry. Ludwig understood in a masterly way how to utilise the skill and knowledge of specialists in those sciences for the purposes of physiology. Hence arose often an interesting relation between teacher and pupil; the pupil himself, sometimes a master in his own particular subject, willingly followed the inspiration of a man who led his talents to new fields of observation. From this happy collaboration a number of papers took origin in Ludwig's laboratory, intrinsically valuable from the accuracy and importance of their histological, chemical, or physical methods, and at the same time full of deep interest from their results in regard to purely biological problems. On the other hand, Ludwig knew how to attach to himself numerous men who wished later on to follow the more practical part of medicine, for the investigations of the Ludwig school were practical in the best sense of the word, being always directed to functions actually occurring in the animal body, and, for the greatest part, best observed through examination of the living animal. A brilliant feature of his teaching was his sketching plans of work for pupils, and in conducting the execution of them. In most cases his worker had to continue where another had left off; and it is a character and peculiar feature of the Ludwig Arbeiten, that not one among them all ends with a summing up of the results gained, but merely with modestly pointing to the new questions arising out of the work done. The very first conference with Ludwig usually showed the pupil

that before him lay a path leading toward something practical with good prospect of success. Ludwig sketched an extended plan and very often entrusted to his pupils the first use of an apparatus which he had himself invented and constructed in view of questions yet to be solved. How much of the actual work he and his able mechanician Salvenmoser did themselves depended on the capability of the pupil. But despite his plan already laid down according to certain lines, and despite his gift of lucid foresight of the nexus of things physiological, the result of the experiment alone was guide for all further steps in the investigation. "The best that we can say is what nature itself says to us;" these words, which his revered teacher said a few months ago to the writer of this paper, were the precept he always followed, a precept which seems a matter of course, and yet is so often subject to the temptation of being neglected.

Ludwig's scientific achievements in physiology must be viewed as to their importance, firstly, for the introduction of new methods; secondly, for their shedding new light on numerous of the most complex processes of the animal machinery. Foremost must be placed the invention of the graphic method. It is only fair to say that Thomas Young, stamping with his remarkable genius each manifold subject that he treated, had before Ludwig invented this method. But it was through Ludwig's quite independent reinvention that it became common property, not alone of physiologists, but also of all other scientific men, of physicists, astronomers, and others. When, in the year 1847, Ludwig studied the influence of the respiratory movements on the circulation in the aortic system, he, for the first time, placed in the open limb of the mercury manometer connected with the artery a float provided with a pen, and let this record the oscillations of the mercury on a drum revolving with a constant velocity. This first recording apparatus—the Ludwig Kymographion—is now a standard apparatus, always applied to if we want to question the throbbing heart, the beating pulse, the breathing breast, and, indirectly, by their answers, the nervous system regulating these wonderful vital actions. The value of this in-

vention cannot be overrated. We younger workers are so accustomed to the graphic method, and look upon it as such a matter of course, that we can hardly conceive the extent to which it has revolutionised biological science. The observing eye replaced the speculating mind in studying functions of animal life, hitherto inaccessible to any experimental research. The course of the most intricate processes and their dependence on various conditions, natural and artificial, could be expressed as measurements of tracings on a surface that could be kept as an objective document for an indefinite time. It is the want of the graphic method in previous generations that accounts for their prevailing ignorance and for their often absurd ideas about the most common actions of life, albeit in those generations there were plenty of mental achievements of the most brilliant kind.

Only three years after the invention of the graphic method, Helmholtz made by its use—besides the appliance of the Jouillet method of measurement of time—his great discovery regarding the velocity of nerve-energy, by which the foundation of a new branch of biological science, namely, that of experimental psychology, was laid.

In close connection with the kymographion, and supplementary to it, appeared in Ludwig's laboratory a great number of other new instruments for estimating time-relations ; these were chronographs, clockworks for making and breaking electric currents at given intervals, the rheometer, used to measure the amount of blood flowing through a vessel in a unit of time, instruments for study of the pulse (sphygmographs), etc. For the examination of the gases in the blood and in the lymph, Ludwig constructed a gas-pump besides various other appliances, which served for a simplification of the measurement of them. The Leipsic physiological laboratory with its complete arrangements for graphic and measuring methods, wherever vital processes, in spite of their subtlety, permitted, has served as model for all institutions since erected. When this year the third International Physiological Congress met in the new and magnificent institution of Berne, Professor Hugo Kronecker, himself a pupil of Ludwig, and universally regarded as the heir of his genius.

for physiological method, could describe this, the youngest Physiological Institute in Europe, an offspring of his great master.

In the last century Albrecht von Haller had formed and, in a certain sense, proved his celebrated doctrine of irritability. It was reserved for Ludwig to shed full light on the deep importance of this principle, and make it available for scientific research by introducing his second great method into physiology. He perfused with blood the organs of the dead animal under the conditions appropriate to each, and awakened in this manner the seeming dead to new life. The excised muscle, heart, lung, kidney, testicle, liver, and the walls of the blood-vessels performed vital processes, the course of which was only subject to the variable conditions of the experimenter. And as evidence of these inner processes, the chemical examination of the perfused blood revealed the interchanges of gases and matter which had taken place between the tissue and the blood, the chemical composition of which had been previously regulated according to the purposes of the investigation. The success of this method not alone proved decisively that each tissue had its own irritability, but it also showed that the entity, which we call life, is composed of a whole set of lives in great extent independent of each other; hereby was opened a new view for the co-ordination of the organism. The analytic method, that most proper to all exact scientific research, was hereby not exactly introduced into biology, but gained its characteristic physiological form. The elementary functions of, for instance, nerve and muscle had been investigated by good methods, and in certain sections of physiology a clearness of description had been developed which could vie with that of physics. But in these investigations a complex of external purely physical and internal semi-vital phenomena had been made to interact so that it remains an open question as to where in the results the line between vital and not vital process should be set. With Ludwig's new method, it was quite different. Here the experimenter simply tried to offer to the isolated organ, as far as possible, the conditions that surround it in the body, and all the results were

phenomena due to the organ itself. Even when an organ was subjected, for the sake of investigation, to conditions not normally existing in the body, the tissues, if they reacted at all, could only react in virtue of their cellular life. This method therefore also benefits pathology in a great measure. Ludwig had an especial liking for this method.

Time was when anatomy and physiology were invariably housed together, and the latter stood to the former in the relation of a servant. No doubt anatomy is the basis of all sound physiological research, but the fetters which chained the younger to the elder science had to be loosed to make its free development possible. It was in the middle of this century that Ludwig together with Du Bois Reymond and Helmholtz sought to give physiology the character of a mechanical science, and thenceforth to physics and chemistry were assigned the leading parts. The adherents of the older morphological method in biology saw with dissatisfaction this new departure, and the feuds between Ludwig and Rudolf Wagner stand out as historic memorials of those days. This antagonism has remained in a certain degree to the present day ; the adherents of morphology who have taken advantage of the theory of evolution and glorying in its recent extraordinary development not seldom reproach the Ludwig school with disregarding the morphological principles of animal organisation. But it was quite erroneous to believe that Ludwig ever relinquished the sure foundation of anatomy. The contrary rather is true, as might be expected of a man who began his career as a teacher of anatomy. The manner in which he differed from previous as from later anatomists has been his new conception of the task of anatomy in relation to physiology. Here he enriched the latter science with a third innovation of method. He started a physiological or, as I should like to call it, a mechanical anatomy. The structure of the tissues and of the organs of the animal body interested him in so far as by their exact description was rendered visible the solution of mechanical problems offered by it. Not the breaking up of a machine into any arbitrary number of infinitesimal particles, not a seeking after the probable origin of the

elementary components—a question of no small interest from quite another point of view—but the building up of single parts into a whole was his mode of describing a machine, thus leading to a knowledge of its powers.

Thus it has come to pass, that the arrangement of the blood-vessels in nearly all the parts of the body has been studied by Ludwig and his pupils, especially in the kidney, the eye, the muscle, the various glands, the small intestine, the ear, the skin and the larynx. His descriptions of the special patterns of circulation special to particular organs awaken distinct conceptions of a most wonderful machinery; he used in his lectures to deduce some of the most intricate functions of the organs from anatomical facts about the vascular apparatus and in a most forcible manner. His theory of the mechanism of the secretion of urine, for instance, is to a great degree based on the insight which his studies on the structure of the kidney afforded him. It was his genius for mechanical anatomy that led him to investigate the lymphatic system, where he showed that with its network of delicate and minute vessels honeycombing the tissues throughout the major portion of the body it forms an apparatus, worked and regulated by mechanical rules. He often used to urge that the anatomy of muscle should be treated as a chapter of dynamics, and the researches of Braune and Fischer have in a very promising manner been conducted according to his principle. His keen appreciation of anatomical facts guided him in devising methods previously unexampled in precision by which light was shed on the functions and structure of the spinal cord, and numerous special centres were discovered in the medulla oblongata. I have tried to show that it was characteristic of him to open out to auxiliary sciences and to their methods distinct physiological applications. No better instance can be quoted than the method, the working out of which he entrusted to his pupil the now celebrated neurologist Flechsig. By this method, his own discovery, Ludwig by means of a pupil obtained new facts of first-rate importance for the physiology of the brain and spinal cord.

Unlike the methods of embryology as handled by mor-

phologists—in Ludwig's developmental method of study of the central nervous system a purely physiological principle is laid hold of, and in a physiological spirit the further researches employing it have been carried on. Flechsig's *Hirn Anatomie* was in later years Ludwig's *Hirn Physiologie*. I well remember how in his lectures he inserted the latest discoveries in the anatomy of the central nervous system with the utmost promptitude. His bent of mind in this direction he reveals exquisitely in his *Lehrbuch*,[1] in a well-known passage which I will quote fully to give at the same time a sample of its amusing and yet powerful style: "Wem die zahllosen Schwierigkeiten vor Augen treten, welche sich hier der Untersuchung entgegen werfen und wer, im Geiste den wunderbaren Bau des Hirns schauend, von Staunen ergriffen ist über die Leistungen dieses zarten und verschlungenen Gefüges, der wird gewiss mit Abscheu sich wegwenden von den rohen Versuchen jener Klasse von Henkern, welche blindlings durch den Schädel hindurch mit Nadeln und Messern in das feinste aller Gebilde stechen und schneiden unter dem dreisten Vorgeben, der Wissenschaft einen Dienst zu leisten. Das Beginnen dieser Hirnbohrer ist kaum weniger sinnvoll als das Bestreben, durch Schusse aus Flinten und Pistolen, die man in eine Cylinderuhr sendet, die Functionen ihrer Räder und Federn zu ermitteln." If we compare the modern delicate methods of experimental research on the brain with those of Magendie, Flourens and others, to whom the just quoted passage refers, we cannot but be grateful to the great man who here saw that anatomy is an essential basis of experiment.

Ludwig was the first to make chemical considerations an integral part of experimental research in physiology. With his accustomed genius for analytical method he contrived to vary the chemical conditions, and thereby induced most important vital reactions. While he left the study of the constituents of the animal body to chemists, he undertook an examination of the chemical processes in the various organs and made these work to his bid and call. Like

[1] Vol. ii., p. 203, 2nd edition. Leipsic, 1858.

physics and anatomy, chemistry also became in his hands a tool for the service of experimental biology.

A record which, in pages preceding mine, Professor Stirling furnishes of the work of the great physiologist, gives the reader an enumeration of the discoveries and researches of Carl Ludwig. In closing my sketch of the master I would pay one more last tribute to his great personality. For us who knew Ludwig in the grandeur of his patriarchal age, "hero-worship" was no idle phrase. He was for us a sage and a priest; he inspired us with warm-hearted admiration for the wonders and beauties which nature willingly reveals when asked with conscientiousness and assiduity; he taught us modesty by impressing upon us how little we knew and how small our powers compared with those of creative nature. But he did still more for us; he, the great investigator of animal life, never forgot its higher, ethical aspects; without any ostentation or pretentious moralising he tried to imbue us with the feeling that knowledge of life, as he understood it, leads to the recognition of a path of duty and of stern ideals. I have read or heard scarcely anything that could vie, for instance, with the simple but beautiful words he spoke to us all once suddenly, when lecturing on pain: "*Der Schmerz ist vielleicht das einzige Reale, was es auf der Welt giebt. Alle Cultur geht darauf aus, das zu beseitigen, was Schmerz verursacht, und die höhere Cultur ist bestrebt das zu beseitigen, was auch nur in der Vorstellung Schmerz erwecken Konnte.*"

LEON ASHER.

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THE PLACE OF RESEARCH IN EDUCATION.[1]

THE address on Art Tuition delivered here a fortnight ago by Professor Herkomer—which I trust many of you had the advantage of listening to—was full of wise counsel, which cannot fail to be of value to those who study it; the more so as Professor Herkomer is not only himself an artist of wide and varied experience, highly gifted with originality, but also an experienced teacher, and is therefore better able to advise than are most other artists. For, after all, only the competent teacher is fully aware of the difficulties which beset the path of the student.

But no advice given by Professor Herkomer was equal in importance to his opening statement—which was subsequently confirmed by members of the governing body

[1] The editor has been good enough to regard the following article as of sufficient interest to warrant its insertion, distinctly with the object of showing that it is desired to deal with subjects likely to attract public attention, so as to interest a wider circle of readers in "Science Progress". I have gladly accepted his hospitality, as I believe it to be of the utmost importance to interest scientific workers as well as the public in questions such as I raise. Unless *we* are prepared to throw ourselves more into such work, there is little probability that the teaching of scientific method will gain that popular recognition which the subject imperatively demands, and which so many of us are persuaded must without delay be accorded to it, if we are to retain any proper measure of national prosperity. It was recently delivered as an address at the Chelsea Polytechnic, and hence it is somewhat egotistical and dogmatic in style; however, I have thought it undesirable to change the style, desiring to lay as much emphasis as possible on the important issues which are considered.

—that in this Polytechnic *a clean beginning has been made in Art;* that you have advisedly elected to be free from all external control and are possessed with the fixed intention of working out your own salvation. Professor Herkomer begged—prayed I may say—that you should be kept clear of all contagion, and all who are your true friends must join in this prayer.

I desire to preach from the same parable as regards the teaching of Science—to exercise the functions of a Medical Officer of Health for Science; but my task is a difficult one. Professor Herkomer spoke to willing ears; his meaning was clear; he was dealing with a popular subject of which we all have some understanding. I cannot but recognise that my subject is generally misunderstood, and its public importance greatly underrated in consequence. Some inscrutable influence has led those who have organised this Institute to appreciate the needs of Art, and having had the wisdom to take proper advice, they have put you in possession of the elements of a perfect system of art sanitation; but the needs of the sister subject Science have yet to be grasped here and elsewhere. The sanitary condition of the dwellings which Science has to put up with throughout our country is most faulty, ill-arranged, out of date, and oftentimes abominable; and if this Polytechnic, indeed Polytechnics and schools generally, desire to place the teaching of Science under healthy conditions, heed must be given to the inspector's warnings.

I might almost take Professor Herkomer's address, write science for art, add a few passages here and there, and redeliver it as my own. Decorative art, that art which enables artists to decorate, you were told cannot be taught on the large scale, it cannot even be taught in schools—it must be taught in the workshop. Decorative science, science which decorates its possessor and enables him or her to be scientific, scientific knowledge which can be made use of in the service of the world, also cannot be studied except in the workshop and in Nature, to whom also the artist must resort. As Professor Herkomer said most truly: all technical education will fail if established on a

scheme in which the master's personality is eliminated, and that must follow in any scheme of wholesale tuition.

But what meaning have the words science and scientific in English ears generally? Do they excite visions of a complicated picture of things concerning our daily life in its minutest details? Certainly not! Their utterance before those who know a little chemistry recalls fireworks and smells and perhaps simple salts; whilst those who take an interest in electricity have thoughts of bells ringing, galvanometer needles wagging, or glowing electric lights; and teachers dream of South Kensington certificates and hardly earnt grants. In the minds of the general public they call forth no response, especially in those of that very numerous section of the community which is concerned in commercial transactions and has no knowledge of manufactures. Science in the eyes of the average Englishman consists of a new-fangled set of ideas, all very well for those who can afford the time to study them, but in his opinion not of such daily practical importance that it is necessary for the nation to pay attention to them. And this unfortunately is the opinion even of "educated" men and of many men of culture. This is perhaps the primary defect in our system to which the Medical Officer of Health for Science is bound to call attention; it is one which we must all unite in overcoming and which Polytechnics such as this should do much to remove.

If public appreciation of scientific procedure can be secured to even a moderate extent, a complete popular victory for those who press for its introduction must soon follow; the advantages to be derived from the general application of scientific method to the affairs of life are demonstrably so great that when once they are made known at all commonly its adoption will be insisted on.

Science is but exact knowledge, and there are as many branches of science as there are of exact knowledge. Remember, however, a loose incoherent body of facts does not constitute a science—a man who is merely possessed of such facts is not scientifically trained. A scientific man is a "knowing man"—not merely a man who knows, but one

who is properly described in the terms of the popular expression—He's a knowing fellow—which implies something more than the mere possession of knowledge, namely, the power to use it properly and with effect. There is every difference, in fact, between the scientific and the merely learned man. To be scientific is to be as far as possible exact in thought, deed, and word; to act with a purpose and after due and careful consideration; to be observant and thoughtful; to be logical and methodical; to be guarded but fearless in opinions and judgment: and it is because we are so rarely all these that we are so rarely truly scientific.

Unfortunately the word science is now associated in the popular mind with certain branches of natural knowledge, and it is because these are generally regarded as of importance only to those whose special business it is to attend to them that the proper application of the term is lost sight of.

I am not here to speak of science teaching—I do not know what that is—but of *scientific* teaching; of the method of teaching scientifically, that is to say, exactly and properly. I am really speaking on the very subject on which Professor Herkomer dilated; we are both pleading one cause although on behalf of somewhat different interests, and mine is the wider plea, and will, in fact, include his. He was the advocate of a practical workshop method of art tuition, under a teacher free as well as competent to consider the peculiar qualities and requirements of his pupils; of a method of so training students as to develop to the utmost their individual innate talents, instead of turning out a set of mechanical automata, blind followers of fashion. I desire to urge that whatever we teach, our method shall be scientific, so that students, in proportion to their abilities, may learn to honestly and usefully apply whatever knowledge they may become possessed of.

Institutions such as this have a great field of usefulness before them if all their work be done from such a standpoint; if it be not they will be absolute and costly failures. I much fear that unless a change in policy, almost amounting to a revolution, take place in many of the schools throughout

the country, devoted to what we are now pleased to call technical education, the results will be disastrous.

Let us consider what has been and is being done. Until about twenty years ago, besides our Universities, the three London Colleges and Owens College, Manchester, the country had little to boast of in the way of institutions for higher learning to which those who had left school could resort; but then important University Colleges were founded in rapid succession in a number of the chief provincial towns. Meanwhile the educational fever spread to London, and there assumed an extremely acute form under the name of Technical Education. The City and Guilds of London Institute was founded, and built first the Finsbury Technical College, and later the Central Technical College at South Kensington, besides establishing an Art School at Lambeth; and by taking over, fostering and largely extending the system of Technological Examinations initiated by the Society of Arts, the City Guilds Institute exercised an extraordinary influence on the establishment and conduct of evening classes for instruction in technical subjects throughout the country. A further development of the same spirit has led more recently to the erection here, there and everywhere throughout London of Polytechnics, etc., and of a large number of technical schools of various degrees of importance in the provincial towns.

Why, it may be asked, all this educational activity, and why especially did the cause of technical education so suddenly spring into prominence? The answer is, you know, because the conviction arose that our manufacturing industries were being seriously threatened in consequence of our failure to sufficiently avail ourselves of scientific aid, and the greater appreciation by foreigners of the services of scientifically trained workers. Because a feeling was abroad such as was graphically expressed by Huxley in a remarkable letter to the *Times* at the close of 1886 in which he pointed out that we had "already entered upon the most serious struggle for existence to which this country has ever been committed," adding "the latter years of this century promise to see us embarked in an industrial war

of far more serious import than the military wars of its opening years. On the East, the most systematically instructed and best-informed people in Europe are our competitors; on the West, an energetic offshoot of our own stock, grown bigger than its parent, enters upon the struggle, possessed of natural resources to which we can make no pretension, and with every prospect of soon possessing that cheap labour by which they may be effectually utilised. Many circumstances tend to justify the hope that we may hold our own if we are careful to organise victory."

The question is—have the steps we have taken to protect ourselves, to hold our own, to organise victory, led to success? In most cases, most certainly not! We are fast proving ourselves to be incapable of holding our own in almost every branch of industry.

Of course there are certain brilliant exceptions to the general rule, but these only prove the rule and enable us to understand the cause of our failure.

Why is this? It is, I believe, because our character is so firmly set that nothing but severe compulsion will lead us to reform; it is because, whatever we may term ourselves politically, we are all by nature ultra-conservative, the rankest political radicals amongst us being the strongest conservatives in their general conduct. It is due to our intense belief in ourselves, the outcome of a long period of unexampled prosperity. We are so intolerantly individual that we cannot bring ourselves to organise and co-operate, and this probably is the main source from which our difficulties spring. Let me take an illustration from agriculture, the most important of our industries, but one which as all know is in a terribly depressed condition. We are told that last year £36,000,000 worth of butter, cheese, eggs, hams, bacon, fowls, ducks, etc., was *imported* into this country! Surely we ought to be capable of producing most of these.

As a matter of fact we cannot even make decent butter or cheese yet. As Sir Henry Gilbert, the distinguished agricultural chemist, remarked to me lately when we were sitting together at dinner: "In our village we prefer to buy Brittany butter rather than the local 'Best Fresh'". If we could

do such things, it would not be necessary for the Yorkshire College, our leading University College, to send out peripatetic teachers to instruct dairymaids, or for County Councils all over the country to do similar work, nor would the Duke of Devonshire have been called on as he was a few weeks ago to open a Midland Dairy Institute. In France they have long known, not only how to make butter properly, but what to do with it when they have made it, an important art which we, from our inability to organise, have also yet to learn. Let me recite my own experience in this matter.

My father was largely concerned with the Normandy butter trade, and on one occasion, about thirty years ago now, I visited France with him; nothing I have since seen has ever impressed me more. The morning after our arrival we were driven out to a market town in the district, where we found all the country folk collected together, each having brought whatever produce they could command to market; our friend, and I should say that the active worker was a woman, went rapidly round the market, tasting each parcel of butter and offering what was thought to be its value, which, of course, was not the retail price of best fresh. If the bid was accepted note was taken by a clerk. On our return, after dinner, in the evening, we found that the butter had not only been collected and brought in, but we actually saw it being carefully mixed and salted and coloured to standard, so as to make it all of one uniform quality, enough to fill a large number of casks being thus dealt with; the next morning it was on the rail and on its way to England. No such thing, I believe, has ever yet been done in this country.

As to eggs coming from abroad, a recent remark made by Sir J. B. Lawes occurs to me: "that it is not that we do not produce them, but we eat them nearly all ourselves". It may be well for us that we do, but the fact is none the less an illustration of the absence from England of thrifty habits such as characterise other nations.

Again, to illustrate why we are beaten by others, let me refer to the fate that has befallen what was formerly an

important English industry—the manufacture of colours from coal-tar, which is now practically in the hands of the Germans and Swiss, so much so that Dr. Caro, the chief living authority on these matters, in addressing the German Chemical Society a couple of years ago was able to refer to it as a German national industry.

It was established in 1856 at Sudbury, near Harrow, by Perkin, who discovered the first aniline colour in the course of a research which he was carrying out, with purely scientific objects in view, under the direction of Hofmann, then Professor in the Royal College of Chemistry, in Oxford Street, London. Soon afterwards the important firm of Simpson, Maull & Nicholson was founded at Hackney Wick—Nicholson being another of Hofmann's pupils. Although similar works were erected in France and Germany, the main business remained in English hands during perhaps twenty years. Meanwhile Dr. Griess—chemist throughout his life to the celebrated brewers at Burton-on-Trent, Messrs. Allsopp—was carrying on researches on diazo-compounds, which he had begun as a student in Germany—one of the most remarkable series of scientific researches ever made; but these did not meet with full appreciation until 1876. In this year the firm of Williams, Thomas & Dower of Brentford introduced certain azo-colours into the market which had been made in their works under the direction of a most accomplished Swiss chemist, Dr. O. N. Witt, strictly in accordance with Griess's prescriptions. The importance of the step thus taken was not fully apparent here, but it was in Germany. Dr. Caro, a member of the now world-renowned Badische Anilin und Soda Fabrik, near Mannheim on the Rhine, who had formerly been chemist to Roberts, Dale & Co., in Manchester—the personal friend of Griess—had been working in the same direction as Witt, and his firm shortly afterwards brought out azo-colours similar to those manufactured by the English firm. This time the seed had fallen upon fruitful soil: the Germans were theoretical as well as practical, and at once saw that the application of Griess's discoveries was likely to be productive of practical con-

sequences. They largely increased their scientific staff—research became the business of the works, and the industry expanded at an extraordinary rate, while the English manufacturers, remaining unteachable, and having no proper scientific staff in their employ, were simply snuffed out.

And the story has yet another side—you have all heard of the turkey-red or madder dyes, formerly obtained from the madder plant which was very largely grown in France, Holland and Turkey. In 1868 two German chemists prepared alizarin, which is the chief constituent of madder, artificially from anthracene—a substance contained in coal-tar. Now Perkin, when a student with Hofmann, had worked with anthracene, and seeing the practical importance of the discovery again set to work and anticipated Graebe and Liebermann in the discovery of a process of manufacturing alizarin. He at once began to make it artificially—his works prospered and during several years the production of artificial alizarin was an English industry. But Perkin made the unfortunate "English" mistake of working almost single-handed — the Germans, meanwhile, were silently but steadily working in their characteristic manner, studying every detail, and soon came to the fore and became masters of the situation.

Perkin's business is now continued as the British Alizarin Company. The conditions under which this firm is working are somewhat peculiar, and such as to procure for it considerable advantages, but the success which has attended its labours is sufficient to show that such an industry might be carried on with special advantage in this country if organised in the proper spirit. Several years ago I had the opportunity of visiting the works shortly after I had inspected the most fully equipped factory of the kind in Germany, and I was agreeably surprised to find that the English works were distinctly in advance of their continental competitors, being able to deal economically with larger quantities. But whereas here the anthracene colour industry is much as it was, abroad it has expanded in various important directions which are proving highly

remunerative, whilst the original madder dyes, although produced in larger quantity than ever, are made at slight profit owing to the excessive competition that has arisen.

Now artificially made dyes have all but displaced natural colouring matters, indigo excepted, even among so conservative a people as our Indian subjects, and the industry is of enormous importance, although not to us. We are so much behindhand in the race that there is little chance of our regaining a good place in the list of runners, even if we go fully into training with that object.

And not only dye stuffs are made from coal-tar. A whole list of substances of the greatest value in medicine are also now prepared from raw materials derived from tar; some of these have proved to be most efficient substitutes for quinine, and the growth of cinchona bark in India and Ceylon has consequently ceased to be the remunerative pursuit it was. All such substances have been the outcome of researches carried out in the German Universities, or in the still more highly equipped laboratories of the German chemical works. Moreover, of late years, Nature's perfumes have one after the other been forced to disclose their character to the pertinacious inquirer, and have been claimed as victims by the chemical manufacturer—abroad, although here again the example was first set by Perkin, who in 1868 showed how *Coumarin*, the odoriferous principle of the Tonka bean, might be artificially prepared.

If we seek to understand our early success as well as our later failure in the branch of industry of which I have been speaking, it is not difficult to trace the former to Hofmann's influence and the latter to our want of appreciation of the inestimable value of the services of such a man. In this connection I may be allowed to quote from my Presidential Address to the Chemical Society in 1894 the following passage in reference to the then recently published memoir by Dr. Caro on the development of the coal-tar colour industry:—

"To those who can understand it, the story told by Caro is nothing less than an epic, but it is one the contemplation of which must in many ways sadden an Englishman,

elevating though it be when regarded from the purely scientific point of view. Full and complete recognition of Hofmann's services to the industry characterises every page of the monograph, and the only ground of complaint which some of us feel that we have against the writer is that his own great services are nowhere referred to. Wherever they received their early training, the true education which experience in the world alone gives was gained by both Hofmann and Caro while in the service of English masters; and it is an interesting problem for speculation whether, had they remained with us, our position would not have been a different one. Germany could scarcely have accomplished what it has done without them, but by years of patient labour her Universities had laid a broad and solid foundation on which alone such men could build. Here, such men had neither brick nor mortar offered to them either by the Universities or manufacturers, and such is our disregard of theory in this country of 'practical men,' that we even now have not learnt the lesson which the contemplation of the success of German chemical industry teaches; shall we ever learn it properly? In London, at all events, we shall probably wrangle during years to come about the establishment of a University worthy of the greatest city in the world, which will set an example and help us again to do our fair share of the work which has been taken from us; and it will be years, apparently, before English manufacturers will all learn to spell the word chemist—and that it will acquire some meaning for them. But it is much to be feared that recantation may come too late, and that the opportunity will have been lost. America, perhaps, will meanwhile have learnt the lesson also, and the competition we shall have to meet will not be European alone; we have not only to go ahead as fast as others, but to make up for much lost time, and it is not likely that others will calmly stand by while we make the attempt.

"Such is the lesson which we may derive, it seems to me, from the study of Hofmann's career and the attendant circumstances; and it is one which we in this Society must take very deeply to heart."

At the present day, no matter what his business, the German manufacturer seeks to understand every detail, and he is always trying to improve his processes. He attains this end by availing himself very fully of the services of men trained *scientifically* at the University, men who have all served their apprenticeship in the school of research ; in fact no man who has not been so trained is looked at now-a-days by the German manufacturer.

In proof of this I may quote words used by Dr. O. N. Witt, now Professor of Chemical Technology in the Berlin Royal Technical High School, the most important institution of its kind in the world, in his report to the German Government as their commissioner at the Chicago Exhibition in 1893. Says Dr. Witt : "What appears to me to be of far greater importance to German chemical industry than its predominant appearance at the Columbian World's Show is the fact which finds expression in the German exhibits alone that industry and science stand on the footing of mutual deepest appreciation, one ever influencing the other; by affording proof that this is truly the case, Germany has given an indisputable guarantee of the vitality of its chemical industries".

Our policy is the precise reverse of that followed in Germany. Our manufacturers generally do not know what the word "research" means ; they place their business under the control of practical men, often admirable men in their way, possessed of much native wit, but untrained and therefore too often and necessarily unprogressive ; and such men as a rule actually resent the introduction into the works of scientifically trained assistants. Hence there is no demand here for men who have been carefully trained as investigators ; consequently our schools do not seriously attempt to train investigators ; in this country such people are only born and grow spontaneously, the high-class manufactured article is made in Germany alone. We elect to sacrifice at the altars of the examination Fiend, for God he cannot be called, and do our best to discourage the development of originality.

Let me give an illustration to make my meaning clearer.

Recently I met a friend who has not only distinguished himself by his intelligent criticism of a particular industry, but has become so interested in it that, having means at his disposal, he has himself become a manufacturer, affording a rare illustration of enterprise. I said: "I trust you are going to work on German lines and engage a good chemist to systematically study your material, and so ascertain how its properties vary with its composition; for I have reason to think from direct experience that much is to be learnt in this way which will make it possible to put the manufacture on a scientific basis". His ready answer was: "Oh, I've got to make the business a commercial success!" Of course I understood what he meant whilst I felt that he could not fathom my meaning—he was too much an Englishman to do that. No doubt he will place his business in the sole charge of a practical man, and as long as it suffices to look only at the surface he will succeed; but then, not improbably, the Japanese will come in and beat him, for they have shown the world that they can organise as well as appreciate scientific method.

Or to give another example showing what may be accomplished under English conditions by adopting foreign methods, let me refer to work done by Mr. Mond, so well known in this country on account of the skill he has shown in developing Solvay's ammonia-soda process. Mr. Mond has long been engaged in seeking for a solution of the problem—how to burn fuel electrically, in such a manner, that is to say, as to directly produce electricity instead of heat. Having improved the gas battery devised in 1842 by the present Sir William Grove, in which hydrogen is burnt electrically, he was anxious to obtain a method of preparing hydrogen readily in large quantities. No good method is known, but a mixture of hydrogen and carbonic oxide is easily made, and even Mond found that on passing this mixture over heated nickel the carbonic oxide was converted partly into carbon and partly into carbondioxide, and as the latter was easily removable he thus succeeded in a measure in effecting his object. In studying the very remarkable action which nickel had on carbonic oxide, it so

happened that on one occasion when experiments were being made in his laboratory the escaping gas was led into the flame of a burner so as to set fire to it, a necessary precaution as the gas is highly poisonous; it was noticed that instead of burning as usual with a non-luminous smokeless flame it burnt with a slightly luminous flame. This strange circumstance led to inquiry being made, and it was eventually ascertained that the metal nickel under certain conditions combined with the gas carbonic oxide, forming a very volatile colourless liquid, and thus one of the most remarkable discoveries of modern times was made. The discovery was communicated to the Chemical Society in 1890 by Mr. Mond in conjunction with his assistants, Drs. Langer and Quincke. Having observed that the compound was very readily broken up into carbonic oxide and nickel, Mr. Mond at once set to work to devise a practical method of preparing nickel on the large scale from its ores through the agency of the new compound, and after spending not only much time and labour, and I believe also a very great deal of money on his quest, was successful in devising a process which he has carried out on the large scale during several months past, and which has enabled him to produce over a ton of metallic nickel of almost absolute purity per week—perhaps the greatest achievement in metallurgy on record. Such action on the part of a native-born English manufacturer is "unthinkable," at least I know of no precedent which would justify us in regarding it as possible under present conditions. I only recently heard of a firm who are doing work of a most important and critical character, involving the expenditure of a very large amount of money, who, having asked an expert whether it would not be well to carefully observe the temperature at which their operations were conducted, on being advised that it was most important to do so, objected that an instrument for the purpose, costing £25, was too expensive to use. The foreign worker would seek to know what happens at any cost.

If the English nation is to do even its fair share of the work of the world in the future, its attitude must be entirely changed—it must realise that steam and electricity have

brought about a complete revolution, that the application of scientific principles and methods is becoming so universal elsewhere, that all here who wish to succeed must adopt them and therefore understand them. It rests with our schools to make the change possible. To repeat what I said in 1894 in the presidential address before referred to: "There can be no question that the future of this country is very largely in the hands of its schoolmasters and schoolmistresses, and there are many among us who are convinced that our progress is much hindered by their failure to feel the pulse of the times—who think, indeed, that they do not suitably prepare the material which we subsequently are called on to mould into its final shape. We look to the new Commission to recommend drastic changes which will enable us to utilise to the full the marvellous ability latent in the English race, and which will help parents in solving the truly terrible problem with which they are confronted in these days of unreasoning and unreasonable competition when the time comes to secure a career for their children. English boys and girls at the present day are the victims of excessive lesson learning, and are also falling a prey, in increasing numbers year by year, to the examination demon, which threatens to become by far the most ruthless monster the world has ever known either in fact or in fable. Ask any teacher who has to do with students fresh from school his opinion of them: he will say that in the great majority of cases they have little if any power of helping themselves, little desire to learn about things, little if any observing power, little desire to reason on what they see or are called on to witness; that they are destitute of the sense of accuracy, and satisfied with any performance however slovenly; that, in short, they are neither inquisitive nor acquisitive, and as they too often are idle as well, the opportunities offered to them are blindly sacrificed. A considerable proportion undoubtedly are by nature mentally very feeble; but the larger number are by no means without ability, and are, in fact, victims of an acquired disease. We *must* find a remedy for this state of things, or perish in the face of the terrific competition now setting in. Boys

and girls at school must be taught from the very earliest moment to *do* and to *appreciate*. It is of no use our teaching them merely *about* things, however interesting—no facts must be taught *without their use* being taught simultaneously; and, as far as possible, they must be led to discover the facts for themselves. Instead of our placing condensed summaries in their hands, we must lead them to use works of reference and acquire the habit of finding out; they must always be at work applying their knowledge and solving problems. It is a libel on the human race to say, as many do, that children cannot think and reason, and that they can only be taught facts; early childhood is the time at which these faculties are most apparent, and it is probably through failure to exercise them then that they suffer atrophy. The so-called science introduced into a few schools in answer to the persistent demands of its advocates has been in most cases a shallow fraud, of no value whatever educationally. Boys see oxygen made and things burnt in it, which gives them much pleasure; but, after all, this is but the old lesson learning in an interesting shape, and has no superior *educational* effect. I would here repeat what I have recently urged elsewhere, that in the future *all subjects* must be taught *scientifically* at schools, in order to inculcate those habits of mind which are termed scientific habits; the teaching of *scientific method*—not the mere shibboleths of some branch of natural science—must be insisted on. No doubt some branch of chemistry, with a due modicum of physics, etc., is the subject by means of which we may, in the first instance, best instil the scientific habits associated with experimental studies, but it must be the true chemistry of the discoverer, not the cookery-book-receipt pseudoform which has so long usurped its place. Whatever be taught, let me repeat that mere repetition work and lesson learning *must* give place to a system of allowing children to *do* things themselves. Should we succeed in infusing the research spirit into our teaching generally, then there will be hope that, in the course of a generation or so, we shall cease to be the Philistines we are at the present time; the education given in our schools will be worthy of being

named a '*liberal education*,' which it never will be so long as we worship the old-world classical fetish, and allow our schools to be controlled by those who reverence this alone, having never been instructed in a wider faith."

And what are schools such as this to do? Should they not also teach in the same spirit? As the Secondary Education Commissioners point out: Education must ever become more practical—a means of forming men (and they should have added, and women) not simply to enjoy life but to accomplish something in the life they enjoy.

To this end, every school, I believe, whether in this metropolis or elsewhere, must work out its own salvation; and we must not look for payment on results, or countenance examinations which reduce all to one dead level.

When Professor Ayrton and I were appointed the first professors of the City and Guilds of London Institute—he having cut his educational teeth in the service of the Japanese and I having been largely made in Germany—we found ourselves in complete agreement that we would have nothing to do with teaching for examinations. Those who afterwards became our colleagues in the establishment of the Finsbury Technical College, my friends Mr. (now Sir Philip) Magnus and Professor Perry fully shared this view, and we all saw that a big problem in education lay before us which we could only work out if we had complete liberty of action, and the Committees we had to do with never for one moment questioned this—all honour to them. I am proud to say that the programmes of the Guilds' Colleges have never been disfigured by references to examinations as objects to be kept in view by students, and I venture to think that when the time comes to consider without prejudice the services which the City Guilds have rendered to the cause of education, it will be admitted that they, more than any other body, have shown true appreciation of English needs. It is worth while noting that although it has never been a coaching college, the Finsbury Technical College has always been overfull, which disposes of the assertion that the bait of an examination must necessarily be held out as an attraction.

But what have the Polytechnics done? To what extent have they made *a clean beginning* in all subjects; and to what extent have they been suborned to worship at the examination shrine to earn the unholy money bribe called payment on results?

I am told that the latter course is adopted in some cases, not because it is felt to be the right one, but because it would not do for Polytechnic B to appear behind Polytechnic A in regard to the number of certificates gained—Governors might object! Unfortunately we know that such arguments are held, that quantity counts for more than quality. The English manufacturer can appreciate a big order, but will not undertake to carry out a small one; and here the foreigner steps in and having made a beginning gradually improves his position until finally he is left in practical possession of the field. Perhaps if we attended more to quality in education there would soon be a large increase in its quantity. A large proportion of those who at present come forward to be prepared belong to Professor Herkomer's great class of those who ought not to be taught at all. Among these are the certificate hunters brought into existence by School Boards and other authorities. There is more than sufficient work to be done among those who are deserving and capable.

To quote Professor Herkomer: "No system could act more perniciously on the morals than payment on results. A few schools have through their strong masters, and in some cases, strong local help, shaken off all the trammels of danger. The fact is, it has all grown into an unwieldy piece of machinery with all the deadening effect of *impersonality* in the teaching. The whole system, when it is not practically upset by a strong and independent master, is lifeless, humdrum, and above all soul-deadening. It is the despair of the masters and the disappointment of the brighter pupils." To all which, I say, Amen!

The system was established at a time when the many schools to which I have referred were unknown, and it was largely because the desired result was not obtained under the system that these new schools became necessary and

were founded. How futile then must be any attempt to base the instruction in these new institutions on discredited methods—such old wine cannot be put into such new bottles. Time does not allow of my fully discussing this matter. I can only point out that the programmes of instruction on which the examinations are based are of such a nature as to make real instruction impossible—even if no other objection could be raised, the extent of ground to be gone over is so great as to make cram an absolute necessity. We have to bear in mind that Germany has prospered without such examinations, Japan also; I believe China is the only country in which a similar system meets with national support—recent events do not encourage us, however, to derive any consolation from this circumstance.

As Professor Herkomer points out: "Granted that the Kensington system was of use once upon a time, and that without it schools of art would not have been established at all, we must look the matter straight in the face and acknowledge that we have now arrived at a point when it must change its form in order to fulfil a great duty and to be of use, or else be disbanded". Undoubtedly this is so, and is as equally true of Science as of Art. The Department has at last perforce itself recognised the necessity of change, but all too slowly, and by appointing a certain number of inspectors has in a measure initiated a new policy. The very distinguished scientific man who is the Director of Science, in his evidence before the Royal Commission on Secondary Education, openly stated indeed that if he had his way he would entirely substitute inspection for examination in the elementary stage—but it is to be feared, unfortunately for the country, that in this, as in most other cases, one swallow does not make a summer.

Professor Herkomer insists that in the future freedom of action must be given to each master—to each town. This independence, he says, is to be obtained only by municipal and County Council aid. "Emancipation from the apron-strings of Kensington through municipal and County Council support would produce *an individuality*," we are told, "*in the art of each town*," for which I may substitute, *in*

the way in which science was taught and applied in each town.

But we must be careful that in leaving the science and art frying-pan we do not jump into a worse municipal fire, of which there is clearly some danger; for while all the world is engaged in decrying examinations, our County Council is bent on devising new ones. Scholarships at times are of great value to students, provided they fall into the right hands, and are obtained as well as held under right conditions. But it is easy to give too many scholarships, and still more so to give them to the wrong persons. Unless the examinations are placed in very competent hands, not only will a serious injury be done to our general system of education by leading those who are preparing boys and girls to adopt methods which it is unwise to follow in schools and to unduly force on their pupils, but the wrong people may be selected; the growth of a class of overtrained pot hunters may be encouraged instead of a vigorous, keen-witted, observant and resourceful race. Those who prove themselves the most apt scholars under the tutelage of the crammer, however able as desk workers, may in the end entirely disappoint the hopes of those who desire most to encourage the development of ability. Huxley has said much as to the importance to the nation of catching the potential Faraday, but it is doubtful whether such would ever shine in a competitive examination in which among other tasks they were asked to write an essay on Oliver Cromwell, or some other like topic equally remote from the daily experience of a healthy lad. If we depend too much on examinations we may easily select the unfittest for the work of the world, and unless very careful we are almost bound to select but one kind of ability—clerical rather than practical ability; unless indeed we altogether change our system of school education, and examine very differently.

It is also difficult to understand what is to be gained by examining candidates for £5 evening scholarships; it must prove to be a very expensive mode of distributing such doles, and it ought to be possible to find some other more prac-

tical way of selecting those whose studies would be materially promoted by such a grant.

Clearly, therefore, it is essential that we should not lose sight of the fact that an exceedingly complex educational system is now growing up under the influence of men who, for the most part, are in no sense experts, and have but little knowledge of the details of such work, although possessed with the desire in every way to do service to the community and to improve our national position. It therefore behoves all who can follow such work to keep most careful watch on the march of events; otherwise those who seek to benefit may in the end do irreparable injury; the present is a most critical period in our history, and such watchfulness is imperatively demanded of us.

I have ventured on this digression because so much depends on the foundation laid at school, as technical studies can only be satisfactorily engaged in by those who have been well trained from the beginning.

As Professor Herkomer says, the kind of individuality to be developed in each town—or in the case of our huge metropolis, in each district—will vary according to the necessities of the community. In future each Polytechnic in London must seek to ascertain what special work it can do to greatest advantage, instead of all following one example, as is too much the case at present. In words almost exactly those of my artist colleague: "This is the only way in which schools will obtain a direct influence over the industries of the country; and the influence will be the right one when the master is carefully selected, because it will be *the school around a man* and not a man struggling to be master in the midst of a system of impersonal teaching, where every student is expected to be squeezed into a great educational mangling machine". "Choose your master carefully," he says, "but then let him *be master*, and he will soon, with freedom of action, vary his forms of tuition according to the idiosyncrasy of each student, or the necessities of his immediate locality. The one true prize to be worked for would be individual progress. All teaching must be on a personal basis."

Choose your master carefully—this is indeed good advice. But this implies, of course, that those who have to choose know how—that they have some standard before them. Have they? Results seem to show that they rarely have. In this matter, as in many others I believe, the City and Guilds of London Institute has set a good example by selecting men known to be capable of doing research work, and a large amount of research work has been done in its Colleges. I am not aware that, excepting in the case of the Principal of this Polytechnic, capacity to undertake research work has been regarded as a qualification; on the contrary, for I know that when it was urged at one of them that a particular candidate had exceptional qualifications of this kind, the answer was: We want a man to teach, not to do research. But the work of true education *is pure research;* really good teachers are engaged in nothing else, being constantly occupied in studying their pupils' idiosyncrasies and in devising suitable methods of instruction. The "researcher" is the equivalent of the artist; the teacher who cannot engage in research is the equivalent of the inartistic copyist. No subject is at a standstill in these days—all progress involves research, although not always original research. The young child even is constantly engaged in research, and the habit is only gradually lost at school under our highly developed modern soul-killing system of perpetual lesson-learning, itself largely devised to satisfy a system of payment on results.

Let us hope, therefore, that every board of Governors will soon learn to appreciate the national importance of research, and will require evidence from every candidate for a teacher's post of ability in this respect—when such is the case, the research spirit will prevail also amongst students generally.

A most desirable example has been set in this direction by what, I suppose, may fairly be termed the least pretentious of the London Polytechnics—that in the Borough Road—which with the assistance of the Leathersellers' Company has just opened a branch tanning school at Harolds Institute, Bermondsey, and with unblushing

effrontery, one may say, prints on the programme under "Tanning School" the words *with special research laboratory*, and not content with this informs us on the next page that "the special research laboratory is fitted up and supported by the Worshipful Company of Leathersellers"—all hail to the Leathersellers, let us say! An industry which makes such a new start—very late though it be—and recognises the fact that research can help it out of its difficulties, is phenomenal in this country, but on the high road to retain its position if not to improve it.

I was much struck at the opening of the school by a statement made by the chairman, Mr. Lafone, M.P., who told us of an American customer who was in the habit of buying large quantities of a particular kind of leather here, of then taking it to America and manufacturing it, returning the goods here for sale. This man had remarked to him, he said, "that he had seen all our works and did not care a fig for our competition—for we had not even begun to know how to make the best". The introduction of the research spirit is sorely needed to cure such an old-world state of affairs as this.

Of course, whenever I advocate research in this way, and urge that the research spirit must be infused into all our teaching as well as into our national life, I am told it can't be done—that children can't solve problems. But there is a saying that an ounce of practice is worth a pound of theory—it is only a half truth, and a saying which is often misapplied, but it consoles me somewhat on such occasions. I *have done it* during the past fifteen years, since the opening of the Finsbury Technical College. No one has the right to say that it cannot be done until they have tried—all who really try, will succeed; those who do not, should not attempt to teach.

H. E. Armstrong.

RECENT WORK ON MOSSES AND FERNS.

THE Archegoniatæ (Mosses and Ferns) hold with regard to the progress of botany a position which might not inaptly be compared with that of the Low Countries in European history. The latter have been the fighting ground of nations: being in themselves objects of desire from their fertility, they commanded even a higher value as controlling the great waterways which lead into the very heart of Europe. And so the Archegoniatæ have been, and still are, the subject of much controversy; however interesting they may be in themselves, as organisms of beauty and of varied life-history, they appeal to the wider sense of the botanist as links of connection between the lower green Algæ, and the higher flowering plants. There is even a further attraction in them, from the fact that plants of this affinity are among the earliest of which we have any trustworthy knowledge derived from the palæontological study of the earth's crust, a branch of botanical science in which advances are rapidly being made. For these reasons, there is perhaps no part of the field of botany which commands more general attention than that which deals with the Mosses and Ferns. It happens that our eyes have been turned afresh to them by the recent publication of a work which, while it adds materially to the store of known facts, also brings forward fresh and weighty points for discussion (*Mosses and Ferns*, by D. H. Campbell: Macmillan & Co., 1895).

The first author who attributed to the Archegoniatæ their true position as connecting links, and showed on a broad footing of observation how their parts may be compared respectively with those of plants higher and lower in the scale, was Hofmeister; and it is not likely that any future writer will ever find it possible to compress into a single volume results which will have so wide and so permanent an influence as his upon this department of botanical thought. By him the comparative method was virtually

established, as applied to the details of development of the Archegoniatæ ; and subsequent work, even the best of it, has done little more than fill in the details of the design which he sketched in its broad outlines. But while Hofmeister, after the manner of a pre-Darwinian writer, proceeded on lines of pure comparison of form and structure, at the present time the tendency is towards a more physiological view. The attempt is being made to distinguish those characters which are most liable to change under varied external conditions from those which are more constant, and so to arrive at a more reasonable understanding of the true relations of organisms one to another as regards descent. What is readily mutable now under varied conditions has probably been mutable in the past also, and will be a less safe character for comparison than those features which persistently retain their form while the conditions vary. We shall see subsequently how important a bearing these matters have on opinion as to the relationships of the Archegoniatæ.

The point which emerged most prominently from the writings of Hofmeister was the phenomenon of "alternations of generations" in the Archegoniatæ. A feature of so wide a bearing as this may, with some certainty, be taken as the morphological expression of some far-reaching causes. However open to the accusation of guess-work, suggestions have been made as to the origin of this wonderful phenomenon, and the hypothetical story may be told in brief as follows :—

The origin of the great green-coloured series of plants, comprising virtually the whole of our land flora, was probably from aquatic forms resembling the green-coloured Algæ of the present day. Among the simplest of these we find that external conditions may directly determine the succession of events ; it has been shown in the case of *Vaucheria*[1] that the operator has it in his power to induce, by

[1] "Zur. Phys. d. Fortpfl. v. Vaucheria sessilis," G. Klebs. *Verh. d. Naturf. Ges. in Basel*, 1895. Also G. Klebs, "Ueber d. Vermehrung v. Hydrodictyon utriculatum". *Flora*, 1890.

exposure to suitable conditions, either sexuality or vegetative propagation; an unlimited number of vegetative or sexual generations may be produced in succession, and the operator may at will stop the succession by inducing under suitable conditions the alternative. Here then is no phenomenon of "alternation of generations" in the true (antithetic) sense, since the very essence of this consists in an obligatory succession of certain phases which are not to be modified at will by varied external conditions.

Such an obligatory succession is, however, found in a rudimentary form in certain other green Algæ, as for instance in *Sphæroplea*, *Œdogonium*, and *Coleochæte*. Here it appears invariably the case that the fertilised ovum (zygote) does not germinate directly so as to produce a single new individual, but there is a division of the zygote into four or more parts; each of these on separation produces by germination a new individual similar to the parent. The *constancy* of this event would point to its having some deep-seated importance; but among these plants the theory is in advance of the facts, and we are waiting for the actual observation of details; it would nevertheless seem probable that the explanation of the constancy is to be sought for in the condition of the nuclei after fertilisation. We know in other more highly organised plants that the nucleus of the zygote is the result of fusion of the male and female nuclei, and that on division it shows, as the result, twice the number of chromosomes which appears in the divisions preceding fertilisation. Such an increase could not be carried on indefinitely through a series of generations, but a return is necessary in some way to the original nuclear condition before fertilisation. In higher plants this has been found to take place by a process of numerical reduction, and it is believed that this process is intimately connected with those divisions which form a constant factor in the life-cycle of such Algæ as those above named.

It may be noted meanwhile, in the absence of direct observations on the Confervoideæ, that peculiar divisions, which may well result in a reduction, are seen on the germination of the zygote in certain Conjugateæ (see

Klebahn, "Studien über zygoten," *Pringsh. Jahrb.*, xxii., p. 415, etc.).

Whatever the details of the process of reduction may be, and we must wait for extended and direct observations to supply them, it would seem probable that the divisions of the zygote in the Confervoideæ are connected with the process of reduction, and perhaps owed their origin to the necessity for such a change. Their existence appears to have supplied the material upon which evolution has worked, so as to result in one of the most surprising phenomena in the whole organic world,—I mean the alternation of generations in the Archegoniatæ. For we learn by comparative study to believe it probable that the originally simple neutral generation became elaborated into the extensive body of the sporophyte ; a stage was thus intercalated in the life-cycle, and becomes more prominent as the series of archegoniate plants is reviewed in ascending order ; the progression culminates in the final triumph of the neutral generation as it is seen in the highest flowering plants.

The circumstances which led to the gradual advance from the few neutral cells which result from the division of the fertilised ovum as in the Confervoid Algæ to the larger neutral generation of the Bryophyta (sporogonium), then to the independent and still more complex sporophyte of the Pteridophyta, seem to have been as follows : Fertilisation is in all these organisms effected through the agency of water ; since in aquatic organisms this may take place at any time, increase in number of individuals *may* take place without intermission ; and this may, and in some cases apparently does, meet the needs for maintenance of the race. But if such organisms migrate to the land, escaping thereby it may be from inconvenient competition, or those conditions being in other ways more favourable, a check would at once be imposed upon unlimited sexuality ; since water would only be present at intervals of rain, dew, or submersion, it would only be possible then to carry out the sexual process. As against the restriction of increase thus imposed, we may recognise the increased number of divisions succeed-

ing fertilisation; an increased number of new individuals being thus produced, while the dryness of the spores when ripe, which is a common character of the large majority of the Archegoniatæ, would facilitate their spread by winds and other agencies. In homosporous forms the spread and survival of the species is secured by production of numerous small spores, rather than by any special development or sexual differentiation of the spores themselves.

But the larger the number of spores produced, the greater will be the demand for nutrition by the parent. This demand we see in part met by the adaptation of the parent sexual plant to the conditions of land-life: the Marchantieæ show this in the elaboration of structure of the thallus which still maintains its simple outline, and it is most interesting to note how, by a perfectly distinct mode of development, a condition is attained in the highest of them (*Marchantia*, etc.) which is not unlike that of the leaves of some angiospermic plants. In other cases elaboration of outward form takes place, as in the Jungermanniaceæ and in the true Mosses (Musci); these sometimes simulating in a curious way the forms of the sporophyte in higher types.

But though the sexual plant might thus become elaborated to subserve more liberal nutrition, this could not suffice altogether, even in cases where a number of female organs had been fertilised on the same individual. An increase of number of spores produced from a single sporogonium necessitates increased facility of conveyance of their nutrition to them during development, and of their dispersion when mature; and in the undoubtedly ascending series of the Liverworts we see most beautifully shown the advance of complexity of the fruit body (sporogonium) to meet these needs; in Professor Campbell's book, pp. 21-151, readers will find the facts collected and arranged so as to illustrate the progression from simpler to more complex types of the sporophyte in these plants. There is good reason to believe, as the result of comparison, that there has been a process of *sterilisation* at work, converting cells which were potentially

sporogenous into cells with a mere vegetative function. For the outer protective wall the evidence of such origin is not conclusive, but for cells lying internally the chain of facts is striking. From the simple *Riccia*, where all the internal cells develop as spores, the most gradual steps of advance lead on to those forms which have a sterile foot, and elaters scattered through the mass of spores; and again, to those in which there is a considerable mass of sterile tissue forming a body like an incomplete columella (*Aneura*, *Metzgeria*, *Pellia*, etc.). The details of these are, however, more completely described by Goebel (*Flora*, p. 1, etc., 1895), and it appears from his drawings that the fertile tissue becomes localised in these forms as an irregular sporogenous band, surrounding the central but incomplete columella of sterile tissue; this is a very material advance in the direction of complexity.

A special chapter is devoted by Campbell to the Anthoceroteæ, which important order he is disposed to remove farther from the Hepaticæ than other writers, and he indicates for it a place "intermediate between them and Pteridophytes". With a simple type of gametophyte, these plants have a sporogonium which consists largely of sterile tissue; it is green, contains chlorophyll, has stomata, and appears thus as an efficient assimilating body. We thus see the neutral generation advanced towards a physiological independence, which is combined with a long-continued growth; for water supply, however, it is still dependent upon the gametophyte, having no root-system of its own. Internally the fertile tissue is more strictly localised than in any other Liverworts; it is referable to a definite layer surrounding the columella, which Campbell shows to be a constant feature. Two points are to be further noted: (1) the archesporial cells are ultimately referable in origin to superficial cells, this is the case also for all Pteridophyta; (2) the whole archesporial layer does not form spores, but sterile cells are intermixed with the fertile, so that the latter may appear in partially isolated groups (*Notothylas*), while the intervening sterile tissue reminds us of the trabeculæ of *Isoetes*.

From what has now been said it will be seen that as regards the sporophyte the Anthoceroteæ show an advance on other Liverworts and approach the Pteridophyta: they appear in fact the most probable link we possess between Bryophytes and Pteridophytes; though we recognise this, the gulf between them is still a very wide and deep one.

The Mosses (*Musci*) have been by various writers held to occupy an important position in the main line of descent of the Archegoniatæ, and it is only in comparatively recent times, and as the result of late work, that the opinion has gained ground that they are, as Campbell has expressed it, "a very clearly defined class, and that their relationship with other forms is at best a somewhat remote one" (*l. c.*, p. 216). "The Mosses, like the foliose Liverworts, seem to represent a modern, extremely specialised type, with no direct connection with higher forms. Undoubtedly related to the Anthoceroteæ through *Sphagnum*, their further development has diverged farther and farther away from the other Archegoniatæ, until in the Bryineæ both gametophyte and sporophyte have little in common with them." The grounds for this change of opinion must be carefully considered.

Since the time of Mettenius the very obvious comparison had been drawn between the protonema of Mosses and the filamentous prothallus of the Hymenophyllaceæ: the delicacy of texture of the leaves appeared to support this comparison. It was extended, on an enlarged basis of facts, by Goebel (*Ann. du Jard. Bot. de Buitenzorg*, vii.); and if, with him, we assume that the oldest archegoniate forms were filamentous, with the sexual organs directly inserted upon a branched cellular filament, it would appear that the Filmy Ferns, and especially *Trichomanes*, approach such a primitive form: at the same time the affinity of these to the Mosses would appear to be indicated, and in *Buxbaumia* Goebel believes that he has found the most primitive form of Moss. As long as we proceed mainly upon the mere formal comparison of the gametophyte, and relegate considerations of the structure of the sexual organs

and of the sporophyte to the background, and pass over the direct effect which external conditions have in modifying the form of the parts upon which our comparison mainly rests, this conclusion may appear satisfactory, and we may continue to regard the Mosses and Filmy Ferns as the nearest points of contact across the gulf which separates the Bryophyta from the Pteridophyta. But before acceptance of this position it will be necessary (1) to be sure that the characters upon which the comparison rests are reasonably *constant*, and (2) to be sure that the characters which we put only in the second place are really less important.

As early as 1888 the want of constancy of the vegetative characters of the prothallus in *Trichomanes* had been remarked upon (Bower, *Annals of Botany*, vol. i., p. 289, etc.). It was pointed out that "abnormal external conditions may largely control the vegetative development of the oophyte" (*l. c.*, p. 292), and that, therefore, in dealing with the decadent sexual generation, even greater caution would be necessary than in comparison of the nascent sporophyte. The inconstancy of form of the protonema in Mosses, and occurrence of flattened expansions in certain species, were a matter of old observation. The opinions based on these and other isolated observations have since been fully justified by the excellent work of Klebs (*Biologisches Centralblatt*, p. 640, 1893): this author has shown by cultures how direct is the influence of light upon the conformation of the gametophyte, and that the filamentous form can be extended or checked according to the intensity of the light to which the growing organism is exposed. We are thus driven to conclude that that which is so readily and directly mutable at the present day cannot be accepted as in itself sufficient evidence of a phylogenetic history which dates back, as the origin of Ferns certainly does, to a time prior to the Devonian period.

The comparisons based upon the filamentous character do not accord with those of the sexual organs; for instance, though the filamentous protonema of *Buxbaumia* may

resemble the filamentous prothallus of *Trichomanes* (Goebel, *Flora*, p. 92, etc., 1892), still the archegonium of the former appears to be a typical Moss archegonium, while that of the latter is as typically that of a Fern. In dealing with the classification of the higher plants the principle has been observed of giving priority to the reproductive organs, and using the comparison of the vegetative organs as secondary. There is no sufficient reason for departing from this method in the present case; if the attention be fixed on the archegonia, the distinction between Mosses and Filmy Ferns remains as wide as ever. Comparative study has taught that the archegonium is a relatively constant organ for the larger groups of plants, and its diagnostic importance should not be subordinated to the claims of the plastic vegetative gametophyte.[1] In addition to this there remains the vast difference between any Moss sporogonium and any Filmy Fern; and when this is added to the difference of archegonia and of

[1] It may here be noted that I have had fruiting specimens of *Buxbaumia* under observation during the summer of 1895. When they came to my hands in May, the sporogonia were of various ages: examination of a number of plants showed that, though the young sporogonia must have been making large demands for nutrition, no chlorophyll-containing tissue was present on those gametophytes which I examined; but there was as usual a very extensive system of rhizoids: there was chlorophyll in the older sporogonia.

Now the sporogonium is a large one, and the question is whence it obtains its nutritive supply. Its own powers of assimilation are not great, if we may judge from the extent of its chlorophyll-containing tissue, though this is larger than in most Mosses. But the question presents itself in its most acute form when the sporogonium is young, and this limited assimilating tissue not yet matured. In the absence of assimilating tissue of the gametophyte (as far as I have been able to observe on my specimens), the only source of supply which I can suggest is from saprophytic action of the rhizoids, a suggestion which Haberlandt has already made as the result of observation of their details.

It is true that after cultivation for some weeks under a bell glass, well exposed to light, there was a slight development of chlorophyll-containing cells on the gametophyte, but as these only appeared subsequently, at a time when the sporogonia were making no further demands for nutrition, they cannot be considered as having any direct bearing on the question. Though not conclusive, the evidence seems strongly in favour of saprophytic nutrition of *Buxbaumia*. F. O. B.

embryology, a very strong body of rebutting evidence appears to oppose the recognition of affinity of Mosses and Filmy Ferns on the basis of the plastic gametophyte.

The Ferns form a natural series (Bower, *Annals of Botany*, vol. iii., 1889). At the one end we find the eusporangiate types with massive meristems and sporangia, the latter being often associated to form synangia; the sexual organs are deeply sunk. At the other end of the series are the Leptosporangiate types, with more definite segmentation of the meristems, attenuated and separate sporangia, and projecting sexual organs. The peculiar parallelism of all these characters indicates that the series is probably a natural one. The question will be, which are the more primitive forms? The only *direct* evidence is from the rocks, and however insufficient this may be, it points clearly to an early preponderance, if not even an actual priority of the eusporangiate type. With this are to be coupled other considerations :—

(1) That as the Leptosporangiate Ferns are the only plants with sporangia originating from a single cell, and since the Lycopods and Equiseta are probably quite distinct stocks, the probability of some at least of the eusporangiate forms having been primitive is a strong one.

(2) That an increasing complexity of form was certainly a leading feature of the progression from any Bryophyte to any Pteridophyte, and the steps from a synangial sorus to a sorus with separate sporangia would have involved a similar increase of complexity of form ; it would be a progression in harmony with the general spirit of the advance.

(3) That those who uphold the Leptosporangiates as primitive have not yet given any reasonable explanation of how that condition came into existence.

(4) That the hypothesis of septation resulting in synangia is in accordance with developmental data ; it is analogous to what is actually seen in the septate anthers of Angiosperms, and it is clearly foreshadowed in the Anthoceroteæ. We are therefore able to give a reasonable suggestion how synangia may have arisen.

On these grounds (and other considerations might also be adduced) it would appear to be a reasonable view that the Eusporangiate Ferns were the more primitive, and that the Leptosporangiates are a later, specialised series, sharing, it is true, with the Mosses certain vegetative characters, though not on that account to be directly connected with them. Accordingly the nearest point of contact of Bryophyta and Pteridophyta is not to be sought for between Mosses and Filmy Ferns—both probably specialised types—but we should rather look to certain Hepaticæ, and compare them with Eusporangiate Pteridophytes. This suggestion came primarily from Campbell (*Bot. Gazette*, Jan., 1890), and the most prominent novelty in his book is the treatment of the whole Archegoniate series with this end in view. This has led to considerable changes in the usual arrangement of the materials, and, in accordance with opinions explained by Campbell in his text, he places the Ophioglossaceæ nearest of Pteridophytes to the Bryophyta. We do not propose here to enter upon a detailed criticism of Campbell's views on these or other questions; they wil doubtless be suitably reviewed elsewhere. We would rather consider the broad results which may be expected to follow from such a change of attitude as he has been the first to advocate, and his book the first to embody in the form of a general statement.

In the first place we see the sporophyte again reinstated in the position which it should hold as an important factor for comparison. The tendency in these borderland speculations has of recent years been to appreciate the decadent gametophyte, and depreciate the nascent sporophyte—as morphological currency. We would not advocate the converse; nor do we wish to see botanists ranged in two camps upholding respectively their favourite generation as the true basis of phylogenetic speculation. What we deprecate is the somewhat unsportsmanlike flight before the difficulties of comparison of the neutral generation. But the fact remains that in the long run it has become the dominant factor in plant-life, and its characters should therefore be given due weight.

Accordingly, while we keep the sexual generation, and especially the sexual organs themselves, constantly in our view, we must address ourselves seriously to reconsider the position as regards the sporophyte. Everybody at the present day is aware of the great differences between the Bryophyta and Pteridophyta. The most essential facts may be summed up in a single sentence; while all Bryophytes have a simple form of the sporogonium, and connected archesporium, Pteridophyta have differentiated vegetative members, and the sporogenous tissue segregated into separate cells or cell-groups. The distinction dates back at least to the period of the primary rocks; obvious intermediate links are absent. On grounds of comparison such as those advanced by Hofmeister, botanists believe that the sporogonium of the Bryophyte is truly homologous with the Pteridophyte plant. But we do not know whether the one was derived from the other; they may represent the results of two parallel but distinct lines of descent from some remote and unknown common ancestor. Thus stated the problem of origin of the more complex sporophyte seems hopeless indeed, for the earth's surface has been almost ransacked; in the last quarter of a century more country has been opened up for scientific inquiry than perhaps in any like period before, and yet no obvious connecting links have been found. As a foundation for opinions we must therefore return to the study of the organisms we know, and especially to those rare, isolated, and presumably antique types, which for various reasons we believe to be nearest to the border line. We must also take into our view such testimony as the rocks are able to provide.

Examining the simpler organisms on the Bryophytic side of the gulf, readers of Campbell's book will clearly apprehend the process which has been at work, how a progressive sterilisation of cells provides vegetative tissue of the sporogonium, so necessary for nutrition and dispersal of the enlarging mass of spores, or, to put the matter in a different terminology, certain cells in the course of descent lose the faculty of undergoing the reducing of chromosomes,

and remain as somatic cells. In the Liverworts as we pass to the higher terms of the series, we see as in the Anthoceroteæ the spore-mother-cells entangled in a mesh-work of sterile cells; here is truly an approach to a septate condition, but even here the final step is not taken.

With these ideas before us we pass to the organisms on the other side, and inquire whether in them there is evidence of sterile cells dispersed through the sporogenous mass: of incomplete septation: of formation of septa in organisms whose near relatives show none: or of actual conversion of tissues, of common origin and position with those which form spores, into sterile masses. Such evidence there certainly is, though much of it is open to various interpretations.

First, it is essential to obtain proof that complete septation can, and does occur. This is shown in the case of certain anthers of Angiosperms: in the Onagraceæ the anther is commonly of the quadrilocular type, but in certain genera, not differing otherwise in vital points, the loculi are found divided by sterile septa into two or more cavities: the tissue which forms the septa has been traced as of common origin with the sporogenous cells. The conclusion is therefore justified that in these cases *sterile partitions have been formed by conversion of potential sporogenous tissue into tissue of the septum.* Such a process is found also in the Mimoseæ, Myrsineæ, Rhizophoreæ, Loranthaceæ, Rafflesiaceæ: it is therefore no isolated phenomenon, but one of wide occurrence in Angiosperms.

The result is a partitioned body, more or less like the synangia which are so common a feature in certain Eusporangiate Pteridophytes, such as *Ophioglossum*, *Danæa*, *Psilotum* and *Tmesipteris*. It would lead us too far to go into the detailed evidence with regard to these plants: it is, or will be, fully set forth elsewhere. From study of various Pteridophyta we arrive at the position that sterile cells are frequently found dispersed through their sporogenous group (*Equisetum*, *Ophioglossum*, *Psilotum*, *Tmesipteris*, compare Liverworts); that incomplete septa, such as those present in certain of the anthers above quoted, are found also in

Pteridophytes (*Tmesipteris*, *Dancæa*, *Ophioglossum*); that potential sporogenous tissue is converted into sterile tissue (trabeculæ of *Isoetes*), and conversely tissue of a septum may become sporogenous (abnormal *Tmesipteris*). From these facts we conclude *that the limits between sterile and fertile tissue are not absolutely fixed, but that the two are mutually convertible;* further, *there is good reason to believe that a progressive septation has played a part in the production of the numerous sporangia of Pteridophyta:* how prominent a part it has played must still remain an open question.

The admission of the process of septation as a probable factor opens the way for fresh views of relationship. During the last quarter of a century various writers have noted the similarity in position of the fertile spike of *Ophioglossum*, and the sporangium of Lycopods: if the latter underwent septation, a body something like the former would be the result. The development has now been found to countenance such a view, while on a wide ground of comparison, morphological, histological, comparison of the gametophyte and embryology (as far as known), it appears that the view is supported. It would not be advisable, however, to carry such speculations further at present, and it must constantly be remembered that other factors exist. It is probable that fusion of sporangia may have occurred in certain cases as exemplified by the ovales of *Loranthus:* it is possible that there may have been in some cases a return to the faculty of spore-production in parts which had previously been vegetative. It must remain for future investigation to decide how far these various factors or others may have been active in producing the numerous sporangia of Pteridophytes as we see them.

But the second great point of difference between Bryophytes and Pteridophytes lies in the absence or presence of appendicular organs. No one can fail to recognise the enormous advantage which accrues from them; they first made physiological independence of the neutral generation possible by providing for successful assimilation, fixture in the soil, and more ready dispersal of the spores when

produced; while permanence by perennation was a general consequence of it.

The importance of this novelty may seem a ready explanation of the absence of the earlier steps of its progress; just as, in the perfecting of bicycles or electric lighting, the earlier improvements soon fall out of the competition, and disappear before their more advanced successors, so with most of the primitive leafy Pteridophytes. However little we may be able to pronounce upon the history of genesis of the primitive leaf (or its *modes* of origin, for they may not all have conformed to the same original type), there is one point which is prominent. There is a certain *correlation* between spore-production and vegetative development; where sporangia are borne, the vegetative development is restricted, but the latter is greater in their absence. Some will say, "This is a mere phenomenon of nutrition," and think that the matter may there be dismissed. But the development of all parts of plants is in a sense "a phenomenon of nutrition," and none the less calls for morphological study. The question will arise with regard to these correlative parts, is either the more primitive foliar form, and if so, which? It has commonly been assumed—in accordance with that uncertain guide, the theory of recapitulation—that the prior existent in the individual (foliage leaf) was the prior existent in the race, and that the sporophyll is therefore an altered foliage leaf. Others may, however, be disposed to hold a converse view, and assert that the foliage leaf is a sterilised sporophyll. There certainly would appear to be a strong element of *a priori* probability in the latter view, in the fact that spore-production was the first object of existence of the sporophyte; the production of spores is the end to which it always tends (leaving on one side rare cases of apospory); and the spore-stage is a constantly recurring event in the life-cycle. This we have every reason to believe has been the case throughout the course of evolution. As regards the history of the sporophyte, spore-production appears to have preceded any vegetative development of the sporophyte.

But in dealing with such a large and complex question,

applicable to so many and varied organic forms, it is not to be expected that all types could be covered by a simple statement; the separate lines of descent should be considered independently of one another, and so a general opinion be gradually formed. As matters stand at present, *it may be concluded with reasonable probability that in certain cases vegetative leaves have resulted from the sterile development of sporophylls.* But even if this were proved to have been their constant and only mode of origin (and we are far from being able to assert this) there would still remain the question of the primary origin of the foliar organs, as also of the root. On these points it must be frankly admitted that we are still in the dark, though their development and comparative study give some clues upon which hypothesis may rest.

We have thus briefly considered the two main points of difference between Bryophyta and Pteridophyta. Little has recently been gained as certain knowledge regarding the increased complexity of form, but the recognition of *septation*, as leading to a segregation of sporogenous masses, introduces an important factor. Though it does not in any sense solve the problem, this acceptance of an idea of septation, coupled with the view that the Eusporangiate Ferns were the more primitive, does appear to be a step towards its solution.

One direct result of Campbell's view of the Eusporangiate Ferns is, so to speak, to pollard the evolutionary tree. In the past, attempts have been made to deduce the various Pteridophyte types from Leptosporangiate Ferns, arranging them in extended sequence; this involved reduction on a large scale, "elimination" of the identity of the sporangium by fusion; the introduction of the idea of "sporocysts," a term applied first to those deeply-sunk sporangia of *Ophioglossum* which were believed to represent a fern sorus, not a sporangium (Strasburger, *Bot. Zeit.*, 1873). Such ideas of reference of the divers stocks to a single type have long been either loosely held, or rejected; but so long as the Leptosporangiates are considered the most primitive, there will always be a disposition to look, in other lines of

descent, for similar simple, and presumably primitive sporangial types. With the acceptance of the Eusporangiate Ferns as the more primitive, this tendency disappears; the large majority of Vascular plants are Eusporangiate; there is therefore no reason to attempt to refer them to any one Leptosporangiate ancestral form. We are prepared on reasonable grounds to recognise a veritable brush of lines of descent. Thus there is no need to look upon *Isoetes* as necessarily a Fern *or* a Lycopod; why should it not be a relic of an independent series not immediately to be ranked with either? The Ophioglossaceæ are not necessarily Ferns; is it not possible that they also may be the result of an independent evolutionary line?

A weak point in much of the discussion of Pteridophyta in the last quarter of a century has been the free resort to hypotheses of "reduction," or "fusion" of parts. The climax of this mode of explanation was perhaps arrived at in the paper above quoted (*Bot. Zeit.*, 1873), in which it is suggested that in the Ophioglossaceæ there has been "a contraction of the sori with reduction of the sporangia," in *Lycopodium* and *Selaginella* a "contraction of the whole fertile leaf segment of the Ophioglossaceæ to one sporocyst," and in *Psilotum* a "contraction of the whole *Lycopodium* strobilus to three sporocysts with reduction of the sterile leaf segment". I quote these as examples of the lengths to which hypotheses of reduction may go, not assuming that their author would uphold them in the present position of the science. In the game of whist the general maxim is sometimes formulated, "When in doubt play trumps;" and so in morphology one may almost recognise as the practice, "When in difficulties suggest reduction". But the strong whist player will only use his trumps with a definite object, and the morphologist should only suggest reduction when there is some reasonable physiological backing for such a suggestion. Hitherto this principle has not been sufficiently observed.

In the Leptosporangiate forms the spore-production is not complicated by the more direct influence of sexuality, nor by any marked need for storage of nutrition. The

chief object appears to be the production of as *many* matured spores as possible. Other things being equal there would thus be an *a priori* improbability confronting any suggestion of reduction ; and on this account we should deprecate any free resort to this as an explanation of morphological difficulties in homosporous forms. It is more reasonable to suggest that it has occurred in heterosporous forms, for in these the tendency is not to mere numerical spore-production, but towards the perfection of a smaller number suitably equipped with nutritious stores, so as to tide over the organism more securely to the time when the embryo can manage for itself. We need not necessarily assert that reduction has never taken place in homosporous Pteridophytes ; other circumstances may have conduced to it. But the dominant feature has no doubt been the production of as many spores as possible, and considering this circumstance we should look critically at any suggestion of reduction in homosporous plants, and only accept it as probable when some good physiological reason can be assigned for its having occurred.

From the above pages it will be seen that opinion on the Archegoniatæ is now in a state of flux. Little is definitely known, and the facts are often susceptible of various interpretation. To those who lay greater stress on one character a certain conclusion may seem clear ; by those who select another character as more important, the converse conclusion may be arrived at, and be held with equal tenacity. The whole would seem to turn upon the selection of the characters to which weight is to be attached. Whatever may be the shortcomings of recent work, and recent argument, one point seems to emerge with clearness. *Relationships, especially among ancient forms, are not to be based upon comparison of characters which are directly variable in a high degree in the individual under changing conditions of present life.* It has been conclusively shown that the vegetative characters of the gametophyte are often so influenced in Mosses and Ferns, and we accordingly conclude with some degree of certainty that relationships based on that foundation are insecure, and especially so

when they do not acquire substantial support from comparison of less variable parts, such as the archspouia. We shall be disposed therefore to agree with Professor Campbell in his main point. But it must not be thought that the priority of Eusporangiate forms is *proved;* few cases are proved or susceptible of proof in morphological discussion. The most that can be done is to show a preponderance of probability, and this we believe has been done in the present case.

F. O. Bower.

THE NEW THEORY OF SOLUTIONS.

(*Continued from vol. ii., p.* 184.)

CONCENTRATED SOLUTIONS.

MOLECULAR weights and concentration. — The extensive measurements which have been made of the molecular weights of dissolved substances are at one in showing that, no matter what method is used or on what property the method depends, the values obtained for the molecular weight of the same substance when dissolved in the same solvent vary with the concentration of the solutions employed. The extent of this variation depends both on the nature of the solvent and the dissolved substance, but for the great majority of indifferent solutions the order of magnitude of the variation is somewhat the same, and is illustrated by the numbers given in the following table. The data, determined by Beckmann, refer to the freezing-points of solutions of alcohol in acetic acid, and show that, as the concentration varies from ·2 gr. to 14·2 gr. of alcohol per 100 gr. of solvent, the molecular weight, calculated by the ordinary freezing-point formula, varies between 47 and 58, the theoretical value being 46.

g	ΔT	M′
·254	·210	47
2·808	2·110	52
6·194	4·490	54
9·687	6·770	56
14·20	9·550	58

Instead of expressing the concentration, as above, in grams per a definite weight of solvent, it may be expressed, as in the case of gases, in grams per a definite volume of solution. With dilute solutions these two methods give practically the same results, and the former, due to Raoult, is almost exclusively used in laboratory methods of determining molecular weights, since the latter, first used

by Arrhenius, involves, in addition to the other data, a knowledge of the density of the solution. In the case of concentrated solutions, however, the two methods may give results which differ considerably, as is shown in the following table. Here are given in the last column the values of the molecular weight of alcohol obtained from the above data on using in the freezing-point formula, instead of g, values of g' or the number of grs. of alcohol in 100 cc. of solution.

P	g'	M″
1·3	·266	47
14	2·853	50·2
30	6·046	50·0
45	9·079	49·8
62	12·65	49·1

The values of M″ now vary by only some 3 units. In general, however, the method of Raoult gives values which vary less with the concentration than those given by the method of Arrhenius. This is well illustrated by the recent work of Abegg (1894) on the freezing-point of very strong solutions. Neither method is theoretically perfect, Raoult's being probably the more correct. It may be taken, however, that the above is a fairly typical instance of the extent to which the molecular weight varies with the concentration.

It is of interest to compare this variation with that shown by the molecular weights of gases at different concentrations. If we calculate the osmotic pressure of alcohol in the above solutions by the equation which holds good for a dilute solution, the values in atmospheres thus obtained are given in the first column of the preceding table. Even in the weakest solution the osmotic pressure is greater than one atmosphere, and in the strongest it is greater than sixty atmospheres. Now it is well known that if, by the equation which applies to a gas of small concentration, we calculate the molecular weight at pressures differing as widely as the osmotic pressures of the above solutions, here also there is considerable variation in the values obtained. This is

shown in the following table based upon observations made by Roth (1880) on ethylene and carbon dioxide. P is the pressure of the gas in atmospheres, g' its concentration in grs. per 100 cc., and M″ the molecular weight.

P	Ethylene.		Carbon Dioxide.	
	g'	M″	g'	M″
1	·1175	28	·1922	44
9·90			2·078	47·6
14·79	1·859	29·5		
29·65	4·137	31·4	7·267	55·4
39·49	5·949	35·4	10·80	61·8
49·34	8·160	38·9	12·05	55·2

The values of M″ are calculated on the assumption that the gas obeys the ideal laws, and, according to the kinetic theory, these laws apply only to gases in which the attractive forces in play between the molecules are inappreciable, and for which the actual volume of the molecules may be neglected in comparison with the volume of the gas. To these disturbing influences, which must of course become more and more important as the pressure increases, are ascribed the variation in the values of M″, and van der Waals' corrected gas equation, which takes note of these influences, approximately expresses the behaviour of compressed gases.

Theory of concentrated solutions.—Although the theory of concentrated solutions cannot yet be adequately tested experimentally, it is of service in explaining some of the foregoing facts. By reasoning similar to that used by van der Waals for gases, it follows that, owing to the attraction of dissolved molecules for each other, the osmotic pressure of a dissolved substance will be less than what it would otherwise be by an amount proportional to the square of the concentration. Acting in the opposite sense to this attraction will be the attraction between dissolved and solvent molecules. The latter attraction will increase the osmotic pressure by an amount which according to Arrhenius (1892) is also proportional to the square of the concentration. This

somewhat surprising result is opposed to the assumptions made by Bredig (1890) and by Noyes (1890) in their theoretical treatment of strong solutions, and is arrived at in the following way. Consider a molecule of dissolved substance in a plane surface separating solvent from solution—such a molecule as would be exerting osmotic pressure on a semipermeable membrane separating the two. Of the molecules attracting the molecule under consideration the solvent molecules in the solution and those in the pure solvent will pull it in opposite directions. Symmetrically situated with respect to the plane there will be a molecule in the pure solvent corresponding with every solvent molecule in the solution, and the attractive effects of these two sets of solvent molecules on the molecule in question will balance each other. Consequently, the only solvent molecules which affect the molecule in question are those in the pure solvent which, as regards the plane, are placed symmetrically to those of the dissolved substance. The number of operative solvent molecules is proportional, therefore, to the concentration of the *dissolved substance*, and, since the total number of dissolved molecules in the plane at any instant is also proportional to the concentration, the total attractive effect between solvent and dissolved molecules is proportional to the square of the concentration. It follows, therefore, that setting aside polymerisation of dissolved molecules, the osmotic pressure P of a solution of volume V at the absolute temperature T is given by an equation of the same form as that of van der Waals—

$$\left(P + \frac{a}{V^2}\right)\left(V - \beta\right) = R'T.$$

Here, however, a, the attraction-constant, is a measure of the difference between the attractions of the dissolved molecules for one another and the attraction of solvent molecules for dissolved molecules; and β is a volume-correction depending on the actual volume occupied by the molecules of the solvent and the dissolved substance, since both of these will diminish the space in which the dissolved molecules can oscillate.

Now, in the case of even a weak solution it is evident that this volume-correction must be considerable. In the case of many liquids it appears that the actual volume occupied by their molecules under ordinary conditions is about one-third of the total volume, consequently, on using such a liquid as a solvent, the space in which the dissolved molecules may oscillate is much smaller than the volume of the solution. Why, then, should solutions conform so closely to the ideal gaseous laws? Why should values of the molecular weight even in strong solutions approximate to the theoretical value? Nernst has pointed out the reason by calling attention to the fact that the properties of solutions have not been investigated to any extent by direct measurements of osmotic pressure, but by indirect methods making use of freezing-point, vapour-pressure, etc. Now these methods are concerned with the separation of solvent from dissolved substance (see vol. ii., p. 184), and although correlated with osmotic pressure in the manner already indicated, they do not in reality give measures of the osmotic pressure but of the osmotic work, or the work required to remove unit volume of solvent from the solution. Let P be the gaseous pressure of a substance, and when the substance is dissolved in a solvent, other things remaining the same, let P be raised in the ratio of $P : P/(1 - \beta)$, where β is the volume-correction due to the molecules of the solvent. Even although this is so the osmotic work will still be P, for the change of volume which the pressure $P/(1 - \beta)$ has to effect is no longer unity but $1 - \beta$. For this reason the volume-correction does not affect the results of freezing-point, vapour-pressure, and other methods based on the same principle. These results are only affected by the attractive forces included in the a-term of the equation, and since the osmotic pressure will be larger or smaller than the theoretical value according as a is positive or negative, it follows that the molecular weight will increase or diminish with the concentration according as the attraction of the molecules of the dissolved substance for each other is greater or less than the attraction of the molecules of the dissolved substance for those of the solvent.

Such is the theory of concentrated solutions which evidently goes some way to explain the facts illustrated by the preceding examples. These show that, in general, the properties of strong solutions deviate from those of ideal solutions just as the properties of compressed gases deviate from those of gases at small concentrations; and further, that the deviations are if anything less marked in the former case, when the comparison is made under conditions which according to the gaseous analogy are similar.

ASSOCIATED SOLUTIONS.

Molecular weights in associated solutions.—To this general rule there exists a well-marked series of exceptions, which are comprised under the heading of associated solutions. The peculiarities of these solutions have mainly been brought to light by determinations of the freezing-point and boiling-point, and were discovered by Raoult. The large number of measurements of the molecular lowering of the freezing-point, published by him in 1884, proved that in solvents such as water, formic acid, and acetic acid, all in-different substances gave what he termed "normal" values; but they also showed that certain compounds, like the fatty alcohols, the fatty acids, etc., when dissolved in solvents like benzene, nitrobenzene, or ethylene dibromide, gave lowerings which were abnormally small. Here are some of the molecular lowerings he obtained calculated from the theoretical molecular weights by the formula $M'\Delta T/g$.

Solutions in Benzene.			
Dissolved Substance.	$M'\Delta T/g$	Dissolved Substance.	$M'\Delta T/g$
Methyl Alcohol - -	25·3	Formic Acid - - -	23·2
Ethyl ,, - -	28·3	Acetic ,, - - -	25·3
Butyl ,, - -	43·2	Valeric ,, - - -	27·1
Amyl ,, - -	39·7	Benzoic ,, - - -	25·4
Phenol - - - -	32·4		

The "normal" value for benzene was 49, hence Raoult came to the conclusion that hydroxy-compounds when dis-

solved in certain solvents, particularly hydrocarbons, gave almost half the "normal" value. Now van't Hoff's theory and subsequent experiments have proved that the molecular lowering of a solvent has only one definite value which can be calculated from its freezing-point and heat of fusion in the manner already shown, and Raoult's so-called "normal" lowerings agree in all cases with the values calculated in this way. The abnormally small molecular lowerings are ascribed therefore to the exceptional behaviour of the dissolved substance, and find a probable explanation in the assumption that in these solutions its molecular weight is greater than the theoretical value. From the above results it would appear that the dissolved substance existed as double molecules; subsequent experiments by Beckmann (1888, 1890), Eykmann (1889), Paternò (1889), Auwers (1893-94), and others, have proved, however, that this view is misleading, because the molecular weight varies as a rule at an abnormally rapid rate as the concentration alters. Here are the results obtained by Beckmann with solutions of alcohol in benzene, the values of the molecular weight being calculated both by the methods of Raoult (M′) and Arrhenius (M″).

g	ΔT	M′	*g′*	M″
·164	·175	45·9	·144	45·8
1·088	·875	60·9	·946	60·2
3·483	1·705	100·	2·952	96·4
8·843	2·725	159·	7·096	145·
14·63	3·445	208·	11·10	179·

In the most dilute solution the theoretical molecular weight is obtained; in the strongest, however, the value is four times the theoretical number. The result arrived at by Raoult regarding the existence of double molecules is therefore due to the fact that he used one solution only, and did not study the effect of concentration.

On contrasting the variation in the values for M′ and M″ in the above table with that of indifferent solutions in general, as typified by the solutions of alcohol in acetic

acid previously considered, it is evident that some additional disturbing factor must operate in the former case, and the data clearly point to the existence in strong solutions of complex molecules of dissolved substance which become dissociated into simple molecules as the dilution increases.

Nature of the dissolved substance.—Now it is one of the most interesting facts discovered in this field that almost all associated solutions are solutions of hydroxy-compounds, and further, that the great majority of hydroxy-compounds when dissolved in hydrocarbon solvents give rise to associated solutions. The following tables, based on data summarised by Auwers (1893) illustrate the behaviour of some of the more important members of four different classes of these compounds, the solvent being benzene. Under g, is given the limits of concentration between which the observations were made; under M′, the observed limiting values of the molecular weight; and under $\overline{M}$, the theoretical molecular weight.

These tables show that in very dilute solutions the alcohols give normal molecular weights which increase at an extremely rapid and approximately constant rate as the concentration increases. The curves obtained on plotting molecular weight as ordinates against concentration as abscissæ are therefore almost straight lines steeply inclined to the horizontal axis. The shape of the curves for the acids indicates that, in general, in extremely dilute solutions the molecular weights would also be normal; the values increase very rapidly, however, as the concentration increases, until they are almost twice as large as theory demands; further increase in concentration is then attended by a slow increase in this double molecular weight of the same order as that usually occurring in the case of unassociated solutions. The curves for the oximes resemble those for the acids with the difference that the increase to the double molecular weight proceeds at a very much slower rate. The two lowest phenols give curves resembling those of the alcohols; the behaviour of the others is normal, although, with the exception of thymol, the effect of concentration cannot be accurately ascertained on account of their limited

solubility in benzene. To summarise, the curves for the alcohols and lowest phenols closely resemble those of complex molecules dissociating into simple molecules, while the curves for the acids and oximes resemble those of double molecules dissociating into simple molecules.

	g	M′	$\bar{M}$
Alcohols.			
Methyl	·32— 7·29	43—144	32
Ethyl	·16—32·45	46—318	46
Isopropyl	·97— 7·39	64—158	60
Octyl	1·52—15·26	138—312	130
Cetyl	1·31— 4·05	229—434	242
Benzyl	·92—20·70	118—271	108
Oximes.			
Acetophenone	·62—28·87	169—279	135
Benzophenone	·23— 1·56	211—260	197
Dextrocamphor	·54—25·50	238—313	167
Laevocamphor	·51—11·77	229—308	167
Dextromenthone	·37— 8·75	228—309	169
Laevomenthone	·58— 7·16	251—292	169
Acids.			
Acetic	·47—22·80	110—153	60
Propionic	1·36—17·68	131—140	74
Caproic	1·05—16·29	194—221	116
Caprylic	1·99—11·39	247—255	144
Lauric	·72—16·30	320—377	200
Benzoic	·27— 4·73	219—236	122
Salicylic	·21— ·40	182—209	138
Phenols.			
Phenol	·34—26·77	144—252	94
p-Kresol	1·15—18·38	107—208	108
Thymol	1·28—17·71	146—184	150
α-Naphthol	·26— 4·99	127—147	144
β-Naphthol	·13— 2·32	125—154	144

Independent evidence that in associated solutions we are really concerned with dissolved substances having abnormally high molecular weights is not difficult to find.

A study of the general physical behaviour of pure hydroxy-liquids, particularly such properties as surface tension, viscosity, vapour-pressure, boiling-point, and heat of vaporisation (see "SCIENCE PROGRESS," vol. iii., pp. 226-230), points to the conclusion that their molecules, as distinguished from those of liquids in general, are really aggregates of gaseous molecules. Hence it is not surprising that in solution there is evidence of these molecular aggregates, and that curves are obtained for their molecular weights at different concentrations resembling those of dissociating gases. Indeed, so definite seems to be the rule that such is the constitution of a hydroxy-liquid, and so uniformly does it give rise to anomalies in certain solvents, that attempts have been made to use observations on the freezing-point of solutions to ascertain in the case of tautomeric compounds of doubtful chemical structure whether or not a hydroxyl-group occurs in the molecule (Auwers, 1894). Why hydroxy-compounds behave in this way is difficult to say. The only hypothesis yet put forward is due to Brühl (1895), who concludes that in the simple molecules of these substances oxygen is tetravalent and that when they exist as simple molecules they are in reality unsaturated compounds. The only other substances which resemble them in giving associated solutions are compounds whose molecules contain a labile hydrogen atom.

Effect of temperature.—Very little work has yet been done to ascertain how the molecular weight of a substance in an associated solution alters with the temperature. The following are Beckmann's values of the molecular weight of benzoic acid determined from the freezing-point and boiling-point of solutions in benzene. The theoretical value is 122.

FREEZING-POINT.		BOILING-POINT.	
g	M′	*g*	M′
·567	223		
1·444	228	1·49	231
2·602	232	3·28	244
4·725	236	5·15	255

In all except the most dilute solutions, the curves obtained on plotting the above values show that the molecular weights determined at the boiling-point are slightly the larger. Here no definite temperature effect is noticeable. No observations at the boiling-point have been made on solutions of the great majority of the other hydroxy-compounds, as they are too volatile to give satisfactory results using the ordinary experimental method.

Nature of the solvent.—Besides the nature of the dissolved substance the second main factor instrumental in producing an associated solution is the nature of the solvent. In general the properties of indifferent solutions give little or no indication of a specific action of the solvent on the molecular condition of the dissolved substance; the solvent apparently merely supplies a medium in which the molecules of the dissolved substance may distribute themselves. The properties of associated solutions clearly prove, however, that in certain cases the solvent cannot be regarded as passive. They show that whereas certain solvents, like water, acetic acid, etc., can resolve the complex molecules of hydroxy-compounds into simple molecules, other solvents cannot effect this decomposition. According to the work of Raoult, Beckmann, Eykmann, Paternò and Montemartini (1894), and Beckmann, Fuchs, and Gernhardt (1895), the following solvents may give associated solutions: benzene, nitrobenzene, paraxylene, naphthalene, diphenyl, diphenyl-methane, methyl-propyl ketone, alkyl bromides and iodides, ethylidene dichloride, ethylene dichloride and dibromide, chloroform, and carbon bisulphide; a less marked tendency to form such solutions is also shown by azobenzene, anethol, and benzophenone. These solvents are mainly hydrocarbons or their simple substitution products which are not of the water-type; other solvents, including, it has to be noticed, hydroxy-compounds themselves, like water, acids, alcohols, phenol, etc., give normal values for all indifferent substances if the solutions be dilute, although in concentrated solutions, even in water, indication of molecular aggregation can be detected (Abegg, 1895).

Little has up till now been done in explaining the dif-

ference in the behaviour of different solvents. Nernst has recently (1894) put forward the view that differences in the values of the dielectric constants of the solvents are an important factor. This idea is based on the fact that the electrical energy of a system of charged conductors is diminished on transferring it from one medium to another having a higher dielectric constant. For this reason, as J. J. Thomson first pointed out in 1893, if a molecule be regarded as composed of charged particles the forces in play between them will be diminished in a medium having a high dielectric constant, and the more readily, therefore, will dissociation occur. Although, as will be evident later, this reasoning can be directly applied to electrolytic solutions, it is only by analogy that it can be used when dealing with the action of a solvent on a molecular aggregate. Nevertheless, where the necessary facts are known, they show that there is a close parallelism between the dissociative action of solvents and their dielectric constants. The following table gives in order of magnitude the values of the dielectric constants at 15°, determined by Landolt and Jahn (1892), and by Thwing (1894), for the liquids commonly used as solvents. The positions of nitrobenzene and acetic acid are somewhat exceptional, yet it is plain that, in general, the solvents in which no association can be detected belong to one end of the series, while hydrocarbon solvents showing definite association fall at the other.

Solvent.	Dielectric Constant.
Water	75·5
Formic acid	62·0
Methyl alcohol	34·0
Nitrobenzene	32·2
Ethyl alcohol	25·0
Ethylene dichloride	11·3
Acetic acid	10·3
Chloroform	3·9
Carbon bisulphide	2·5
Paraxylene	2·4
Benzene	2·2

According to Brühl the reason why oxy-solvents do not as a rule yield associated solutions is due to their unsaturated nature; oxygen is here regarded as playing the part of a tetravalent atom, the "free valencies" of which being the real cause of the dissociative action of the solvents on molecular aggregates.

Comparison of abnormal molecular weights given by different methods.—It follows from the preceding discussion that, assuming Avogadro's hypothesis applies to dilute solutions, in certain solvents the molecular weights of certain dissolved substances are not identical with their gaseous molecular weights. The alcohols which have normal gaseous molecular weights give abnormal solution molecular weights. Both the gaseous and solution molecular weights of the acids are abnormal, but the two are not identical under conditions which, according to the gaseous analogy, are similar. Thus Nernst calculates that in solution in benzene, at 80°, acetic acid is dissociated to a greater extent than when it exists as gas at the same temperature and concentration; the reason he gives being that the dielectric constant of benzene (2·2) is greater than that of free space (1). This reasoning would not apply, however, to the molecular weights of the alcohols in solution and in the state of gas. Indeed, if we deal with substances which give abnormal molecular weights when gasified, in no case are these identical with the solution molecular weights, and the latter vary with the solvent. For example, chloral hydrate and alcoholate are partly dissociated on vaporisation, but when dissolved in benzene they give practically normal molecular weights in dilute solutions. In acetic acid they are partly dissociated into their proximate constituents in dilute solutions, and in water chloral hydrate gives normal values while the alcoholate is almost completely dissociated at all concentrations up to 18 per cent. The conclusion to be drawn from the foregoing facts is, therefore, that if the dissolved substance has an abnormal gaseous molecular weight, or if in the liquid state it consists of molecular aggregates, the solution molecular weight differs from the gaseous molecular

weight. These two values agree, however, if the substance when gaseous and when liquid consists of simple molecules.

It is also of interest to compare the molecular weights obtained from associated solutions with those determined by independent methods for the liquid molecular weights of the pure dissolved substances. The only property which has to any extent been used to give an idea of the degree of molecular aggregation is surface energy. From measurements of this property Ramsay and Shields (1894) conclude that at 20° the average liquid molecular weights of methyl alcohol, ethyl alcohol, and acetic acid are respectively $(CH_3OH)_{2\cdot32} = 74$, $(C_2H_5OH)_{1\cdot65} = 76$, and $(CH_3COOH)_{2\cdot13} = 128$. On referring to the table on p. 385, it will be seen that although in benzene the solution molecular weights of acetic acid are not far removed from the above value, even in moderately dilute solutions the molecular weights found for the alcohols are much larger than those given for the pure liquids by surface-energy observations. A more extended comparison of the two sets of numbers shows that, in general, the results are not in harmony; as a rule the solution molecular weights are the larger, and hence if the liquid molecular weights deduced by Ramsay and Shields are correct, in most cases the solvent cannot be regarded as merely breaking up the complex molecules of the pure liquids into simpler molecules, but as promoting further aggregation. In a few cases the opposite effect is exerted. Thus, according to surface-energy observations, acetone, nitroethane, and propionitrile are associated liquids, but in benzene they give normal molecular weights. Of course the agreement between the results of the two methods could not be otherwise than imperfect in the present state of the questions. Because, on the one hand, the values of the molecular weight given by strong solutions are not the result of molecular aggregation alone, but are affected by the attractive forces already considered; and, on the other hand, the molecular weights derived from surface-energy observations can hardly be regarded as anything but approximations, since the method of calculation is not free

from objection, and does not rest on a firm theoretical basis (see "SCIENCE PROGRESS," iii., 229). What appears conclusively settled is, that dissolved substances producing associated solutions when pure and in the liquid state consist of complex molecules; and one interesting point, at present in an unsatisfactory state, is whether the aggregation of the liquid when pure is not always greater than when it is dissolved.

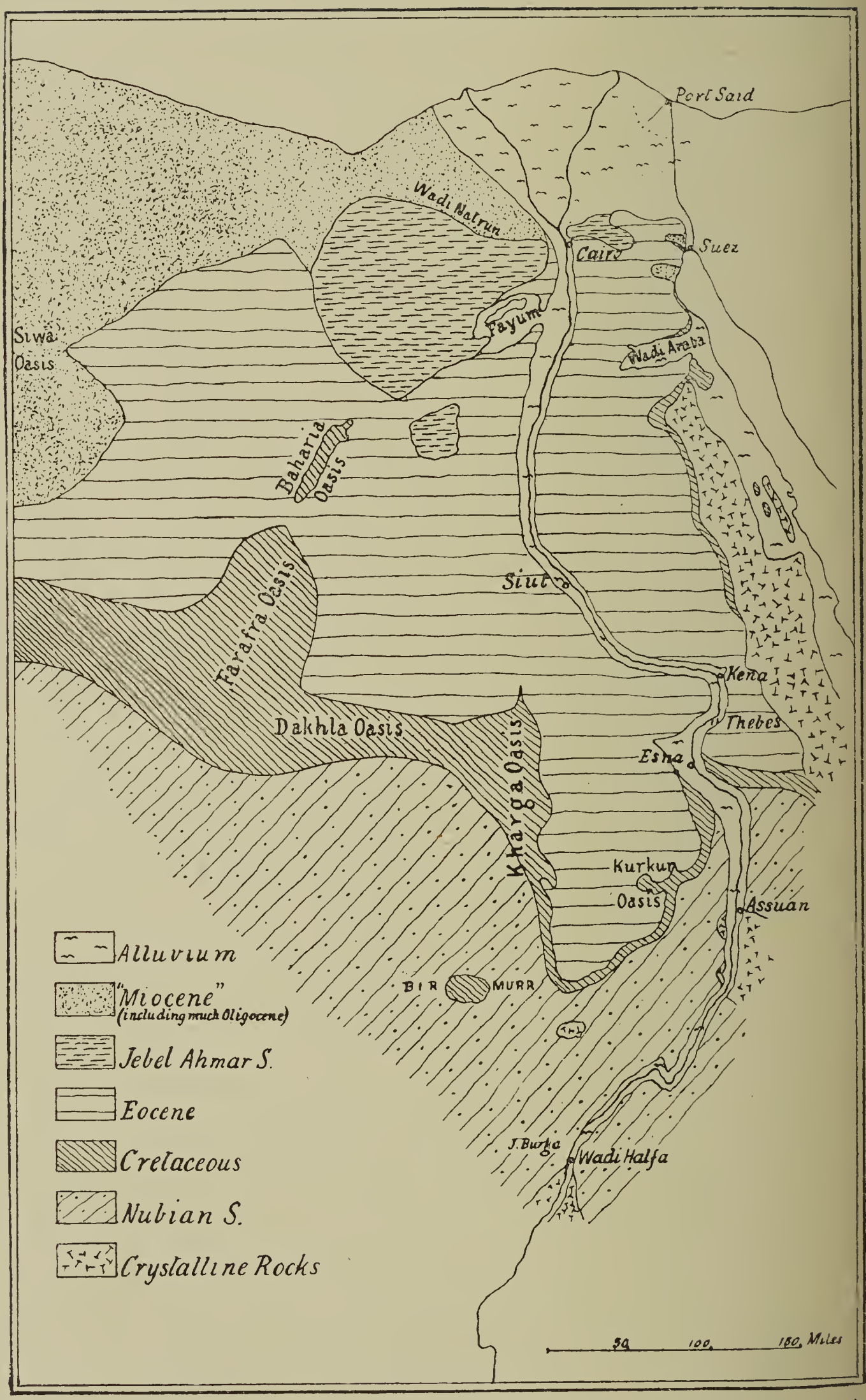
Anomalous freezing-points.—It sometimes happens that, as regards their freezing-points, certain solutions simulate the behaviour of associated solutions. This is due to the fact, first pointed out by van't Hoff, that, on freezing the solution, dissolved substance solidifies along with the solvent, and of course in this case the ordinary freezing-point laws are no longer valid. In this way have been explained the low values of the molecular lowering, and high values of the molecular weight found for m-kresol in phenol, and aldoxime in acetoxime, observed by Eykmann (1889), and thiophen in benzene observed by Paternò (1889). It has further been shown that the tendency to form mixed crystals on freezing is most marked in the case of isomorphous substances, and Kuster's work (1890-91) has proved that the freezing-point of an isomorphous mixture may in some cases be calculated from those of its constituents by the ordinary admixture rule. In such a case, if the freezing-point of the dissolved substance is higher than that of the solvent, its presence will bring about a rise instead of a fall in the freezing-point of the solvent. This behaviour was observed by Heycock and Neville (1889) in the case of a solution of antimony in tin, and by Eykmann for β-naphthol in naphthylamine. Van Bijlert (1891) in these and other cases proved by actual analysis that the ice separating from the solutions really contained dissolved substances.

An interesting development of this question is due to Ferratini and Garelli (1893), who discovered several anomalous solutions of this class, and pointed out that in all probability similarity in the chemical constitutions of solvent and dissolved substance, rather than similarity in their

chemical behaviour, led to the formation of mixed crystals. For example, the molecular weight of quinoline is normal in benzene, but abnormal in naphthalene; acridine gives a normal value in benzene, but an abnormal value in phenanthrene. Such observations may be used to throw light on the chemical constitution of dissolved substances. Nicotine, for instance, unlike γ-dipyridyl, does not form mixed crystals with diphenyl, consequently the constitution of nicotine, which is at present undecided, is probably of a different type from that of γ-dipyridyl (Garelli, Ciamician, 1894).

J. W. Rodger.

(To be continued.)



GEOGRAPHICAL SKETCH-MAP OF EGYPT. (Principally after Zittel and Lyons.)

THE GEOLOGY OF EGYPT.

IN the account of the geology of the Sahara which appeared in "SCIENCE PROGRESS" for July, 1895, the description of the Egyptian deposits was necessarily very cursory. But Egypt is geologically a part of the Sahara, and no account of the latter can be complete without including the former. The present article, therefore, is practically a continuation of the previous one.

The literature of Egyptian geology is so extensive that it would be impossible, even if it were desirable, to attempt here a complete bibliography of the subject. I have, therefore, quoted only those works to which it is necessary to refer in order to give a fairly complete description of the geology of this region. Even yet by far the most detailed general account of the country is to be found in Zittel's *Libysche Wüste* (34). But this was published in 1883, and since that time much geological work has been done in Egypt, especially in Lower Egypt and in the Arabian Desert. References to the most important papers which have appeared since the publication of Zittel's work will be found in the bibliography at the end of this article.

Egypt, in the ordinary sense of the term, includes the region which lies between the sandy part of the Libyan Desert on the west and the Red Sea on the east, and which extends from the shores of the Mediterranean to the "Second Cataract" near Wadi Halfa. It is divided into two parts by the Nile; and excepting the delta and the valley of that river, it is for the most part a barren waste of mountain and plateau. West of the Nile we have the Libyan Desert, and east of it the so-called Arabian Desert extending to the Red Sea.

North of Cairo the greater part of the country is low-lying, and much of it is of recent origin; but south of that town there is on each side of the river an extensive plateau of Eocene rocks. The western plateau is the larger and more monotonous in character, and extends as far south as

the line of oases of Upper Egypt, where it ends abruptly in a steep escarpment looking southwards. The belt of comparatively low and fertile country in which the oases lie is made of Cretaceous beds, and stretches in a general W.N.W. direction from a point some distance west of Assuan. South of the oases the ground again begins to rise, and forms a second undulating plateau which extends southwards far beyond the boundary of Egypt.

The geological structure on the west side of the Nile is very simple. The formations succeed each other in regular order from south to north, and it is clear that there is a general dip towards the north. But this general simplicity is complicated by various minor anticlinal and synclinal folds, which Lyons (16) has attempted to reduce to a system. The evidence in such a region as the Libyan Desert is necessarily incomplete, but he points out that the principal wells occur along certain lines which run approximately from W.N.W. to E.S.E., nearly parallel in fact to the line of the Upper Egyptian oases. He shows, too, that in one case the line coincides, for a part of its course at least, with an anticlinal axis; and he comes to the conclusion that the other lines also are probably anticlines.

He does not find these lines upon the Eocene plateau, where, indeed, there are no wells; but he believes the Farafra and Baharia oases to be upon an anticlinal axis crossing the others nearly at right angles.

The physical structure of the Arabian Desert is much more complicated. West of a line joining Suez with Kena there is another plateau of Eocene rocks, which is, however, much more cut up by valleys than the Libyan plateau. The most interesting, geologically, of these valleys is the Wadi Araba, which lies between the Northern and Southern Galalas, and opens into the Gulf of Suez about latitude 29° N.

East of this plateau there is a mountain chain which runs nearly parallel to the Red Sea, and consists of a central core of crystalline and schistose rocks, flanked on each side by Cretaceous and later deposits.

The geological formations which are represented in Egypt are not very numerous. For convenience of description they may be grouped as follows :—

Crystalline and schistose rocks.
Nubian Sandstone.
Cretaceous.
Eocene and Oligocene.
Miocene.
Pliocene and recent.

Crystalline and schistose rocks.—The crystalline and schistose rocks which form the basis upon which rest the sedimentary formations of Egypt, are first exposed in the bed of the Nile near the First Cataract at Assuan. Many writers have referred to them more or less briefly, but they have never been very carefully examined. Hawkshaw (13) has given a map of the area, and Dawson (9) has divided the rocks into two series which he correlates with the Laurentian and Arvonian ; while Bonney (6) and Miss Raisin (21) have described their petrological characters. Dykes of diorite appear to be abundant.

According to Floyer (10) the crystalline rocks which form the core of the range of mountains along the shores of the Red Sea, have metamorphosed the sandstone in contact with them ; and according to Johnson (15) there is a similar alteration around the granite near Kalabsheh, above Assuan. Much doubt has been cast on these statements ; but it should be remembered that most large crystalline masses consist of rocks of various ages, and some of these are usually intrusive. Moreover, the "sandstone" which is in contact with the crystalline rocks is probably not everywhere the same age. Some of it is almost certainly Palæozoic and some of it Cretaceous.

Nubian Sandstone.—The "Nubian Sandstone," as the term is generally used in Egypt, includes the whole of the beds which lie between the crystalline rocks and the fossiliferous Cretaceous series. At Assuan it rests upon gneiss and schist ; but a little west of the Nile it extends much further south, and occupies in fact nearly the whole

of the country from the Egyptian oases to Khartum. It rises gradually from beneath the Cretaceous belt of the oases, and forms a barren undulating plateau upon which stand a number of low flat-topped or pyramidal hills.

This, the principal area of Nubian Sandstone in Egypt, extends only a short distance to the east of the Nile; but similar beds underlie the Cretaceous of the Wadi Araba in Middle Egypt.

The characteristic rock of the series is a dark sandstone with a siliceous cement, coloured by iron and manganese. It is often very hard and durable and has yielded much of the stone of which the temples of Nubia and Upper Egypt have been built. But the series is not so homogeneous as its name seems to imply. Lyons (16) describes beds of clay in it, sometimes of considerable extent; and also deposits of iron ore. The latter contain fragments of fossil wood and are believed to have been formed in marshy lagoons.

With the exception of fragments of silicified wood, referred to *Nicolia*, *Araucarioxylon* and other coniferous and dicotyledonous genera, fossils are extremely rare. Several observers have discovered casts of shells, but they are generally indeterminable. Coquand (7), however, has described a specimen from Assuan which he refers to the Upper Cretaceous species *Ostrea Verneuili*.

Until recently this was all the palæontological evidence that we had as to the age of the Nubian Sandstone in Egypt, and it was chiefly because the sandstone is conformably overlaid by the Upper Cretaceous that Zittel (34) considered it to be of Cenomanian age. In the Wadi Araba, however, in beds which were formerly referred to the Nubian Sandstone, Schweinfurth has discovered a fossiliferous band some distance below the Cretaceous; and from this he collected a number of fossils which he at first supposed to be Devonian (25). Further examination (27) has shown the fauna to be a Carboniferous one, and Walther has referred it to the Carboniferous Limestone (32). Among the fossils are such forms as *Productus semireticulatus*, *Streptorhynchus crenis-*

tria, etc., so that the Carboniferous age of the deposit is unquestionable. But Schellwien (22) has recently undertaken a re-examination of the specimens, and comes to the conclusion that they belong to the Upper Carboniferous. He finds two species of *Enteles* [*Enteletes*], a species of *Spirifer* allied to *S. mosquensis*, and other forms characteristic of the Russian facies of the Upper Carboniferous. In fact, according to Schellwien, there is not a single species present which is confined to the Lower Carboniferous, while there are several which have never been found except in the Upper Carboniferous.

However this may be—and the occurrence of *Enteletes* is strong evidence in favour of Schellwien's views—the fact remains undisputed that a part of what was formerly known as the Nubian Sandstone in the Wadi Araba, is of Carboniferous age. The bed in which the fossils were found is succeeded, without any apparent unconformity, by marls and sandstones which are themselves regularly overlaid by undoubted Cretaceous beds with *Exogyra*. Some fifty metres above the Carboniferous fossil bank there is a red sandstone which contains *Araucarioxylon*.

Walther divides the whole of the old "Nubian Sandstone" of the Wadi Araba into three groups :—

1. 100 m. of sandstones and marls which are pre-Carboniferous [or in part Lower Carboniferous].
2. 20 m. of marl and limestone which must be referred to the Carboniferous Limestone [to the Upper Carboniferous].
3. 200 m. of sandstone which may be considered as the equivalents of the Permian, Trias, or Jura.

In Syria and the Sinaitic peninsula also the Cretaceous is underlaid by a sandstone which is generally known as the Nubian Sandstone; and below this at one or two localities there is a limestone band containing Carboniferous fossils (3, 14, 29). The limestone is itself underlaid by another group of sandstones and conglomerates. The sequence, therefore, is precisely the same as in the Wadi Arabi.

By many writers the term Nubian Sandstone is now restricted to the part of the series which lies above the

Carboniferous band, and clearly this must be post-Carboniferous and pre-Cretaceous, or possibly early Cretaceous. But we can say little beyond this. If the apparent absence of unconformity is to be taken as evidence, the whole of the period between the Carboniferous and the Upper Cretaceous must be represented; and this is scarcely probable.

There is nothing to show which part of the Wadi Araba sequence corresponds with the Nubian Sandstone of the Libyan Desert, excepting only the occurrence of *Araucarioxylon*—a genus of great vertical range. Lyons remarks that he is unable to distinguish any divisions in the sandstone of the Libyan Desert, and refers the whole of it to the Cenomanian. But in so extensive and so imperfectly examined an area, negative evidence can go for little.

Cretaceous.—The Cretaceous rocks form the floor of the fertile belt in Upper Egypt, which includes the Farafra, Dakhla, Kharga, and Kurkur oases. In the case of the Dakhla oasis it also forms the northern wall. The Baharia oasis, which lies in a depression in the Eocene plateau, is also cut down to the Cretaceous rocks below. South of the Kharga oasis there are one or two outliers resting upon the Nubian Sandstone. Bir Murr, for example, is stated by Lyons (16) to lie in Cretaceous rocks; and the Selima oasis, S.W. of Wadi Halfa, is also believed by him to be Cretaceous.

About a dozen miles west of the great pyramids there is a rhomboidal area of Cretaceous rocks, which has been brought into its present position among the Tertiary deposits by faults along its four sides. This area has been described by Mayer-Eymar (17), Schweinfurth (28), and Walther.

On the east side of the Nile the Cretaceous forms a belt which lies along the western flank of the mountain chain near the shores of the Red Sea. It has also been found in patches on the eastern flank. It occurs, too, in the Wadi Arabi, and further north still it forms the northern foot of Jebel Attaka, the mountain which is so conspicuous an object from the port of Suez.

The Cretaceous of the Upper Egyptian oases has

been fully described by Zittel (34), who finds that it consists of three main divisions, *viz.* (in descending order)—

3. White well-bedded limestone or earthy chalk.
2. Greenish and ash-grey shaly clays.
1. Beds with *Exogyra Overwegi.*

The *Overwegi* beds, which rest with perfect conformity upon the Nubian Sandstone, are some 130 m. thick. In the lowest part of the series the predominant rock is a brown iron-shot sandstone, in which fish teeth occur in great numbers, and fossil wood is also occasionally found. The teeth belong to the genera *Otodus, Lamna, Corax* (*C. pristodontus*), etc. The middle beds are for the most part variegated clays, with banks of sandstone, marl, and limestone; and it is here that the characteristic fossils of the series, such as *Exogyra Overwegi* and *Inoceramus Cripsi* var. *regularis*, are most abundant. The upper beds, which are chiefly laminated clays, are relatively poor in fossils.

The shaly clays which overlie the *Overwegi* beds are but little different in appearance from those which are found in the *Overwegi* series itself. Besides gypsum and rock-salt they contain also brown iron-ore, and iron pyrites superficially oxidised. Fossils are not abundant except in certain beds, and are fairly distinct from those of the *Overwegi* stage. Among them are *Nautilus danicus*, *N. desertorum*, etc.

The highest beds of the Cretaceous consist of well-bedded chalky limestone, often somewhat marly at its base. It joins the top of the escarpment which rises on the north side of the Dakhla oasis, while in the Kharga oasis it is itself capped by the Eocene limestones. In the oasis of Farafra it shows all the characters of a true chalk, and consists almost entirely of Coccoliths and Foraminifera. Among the chief fossils which have been found in it are *Ananchytes ovata*, *Gryphea cf. vesicularis*, *Pecten Farafrensis*, etc., and numerous fish teeth.

Many of the species which occur in the Egyptian Cretaceous are new; but the presence in the lower beds,

of the very characteristic *Exogyra Overwegi*, suggests that the whole series must belong to the Upper Senonian, for in Tripoli *Exogyra Overwegi* is associated with *Ostrea larva*. Others of the common fossils, such as *Corax pristodontus*, *Nautilus danicus*, etc., are also Senonian forms, and the whole fossiliferous Cretaceous of Upper Egypt is correlated by Zittel with the Upper Senonian or Danian. This view is further supported by the fact that there is no break between the chalky beds at the top of the series and the nummulitic rocks above.

The floor of the Baharia oasis, which lies some distance north of the great belt of oases, is formed of sandstones, marls, etc. They present a considerable resemblance to the lower part of the *Overwegi* stage, but are succeeded by nummulitic limestone at so short an interval as to preclude the possibility of the presence of the higher Cretaceous beds except in a very attenuated form; and hence Zittel was forced to the conclusion that they must be of Tertiary age. Lyons, however, has since discovered *Exogyra Overwegi* in these beds, and thus shown them to be Cretaceous (16).

As yet there is no proof on the west side of the Nile of the existence of any Cretaceous rocks older than the Senonian. But in the Arabian Desert east of that river the Senonian is underlaid by other beds containing a rich and varied Cenomanian fauna.

Near the Monastery of St. Paul, south of the Wadi Araba, Schweinfurth finds a reddish-brown sandstone resting directly upon the crystalline rocks. This is succeeded by an alternation of marly and calcareous beds in which *Hemiaster cubicus* occurs in profusion. Other common fossils are *Sphærulites Schweinfurthi*, *Exogyra Africana*, *Plicatula Fourneli*, *Ammonites Mantelli*, etc., and the whole assemblage can be matched in the Cenomanian deposits of Algeria, Calabria, and other Mediterranean districts.

In some localities these beds are immediately succeeded by a white chalky limestone; but in others, for instance, at the Monastery of Saint Antony in the Wadi Araba, there is an intervening series of variegated marls with inter-

bedded limestones, which contain *Ostrea larva*, *O. serrata*, *Gryphea vesicularis*, etc., and are clearly of Senonian age.

Further north, at the foot of Jebel Attaka, Vaillant (31) long ago discovered a series of Cretaceous rocks. They consist chiefly of limestones, generally dolomitic, and in these he discovered two fossiliferous bands. The lower contained *Hippurites cornu-vaccinum* and *H. organisans* and the upper included *Ostrea larva*, *Janira sexangularis*, etc. The former is very probably Turonian and the latter Senonian. Here, as elsewhere in Egypt, the Cretaceous is succeeded conformably by the Eocene.

Eocene and Oligocene.—The Eocene is by far the most widely spread formation in Egypt. On the west side of the Nile it forms the extensive plateau which reaches from the oases of Upper Egypt northwards as far as Cairo and the Great Petrified Forest. In the Arabian Desert it occupies most of the space between the valley of the Nile and the crystalline core of the Red Sea Mountains; the eastern boundary nearly coinciding with a line joining Suez and Kena (23).

The formation is divided by Zittel into three stages:—

Upper Eocene stage.
Mokattam stage.
Libyan stage.

The Libyan stage forms the southern part of the great Eocene plateau and crowns the escarpment which overlooks the Farafra and Kharga oases. Lithologically there is no definite line between it and the Cretaceous below; but the fossils are very different. The Libyan beds contain *Operculina libyca* and nummulites, which in this region have never been found along with Cretaceous fossils.

The older division of the Libyan stage is well seen in the southern escarpment of the Eocene plateau, and also in the valley of the Nile between Esna and Thebes. It consists chiefly of calcareous rocks, the lowest beds being sometimes marly or sandy. The most characteristic fossil is *Operculina libyca*, which sometimes fills whole banks. Various nummulites also occur, such as *N. Ramondi*, *N.*

Guettardi, *N. Biarritzensis*. Gasteropods and lamellibranchs are abundant, and sea-urchins of various kinds. More than two-thirds of the species are said by Zittel to be limited to the Libyan beds; but there are sufficient which are found elsewhere to determine the geological age with some accuracy. The three nummulites mentioned occur in the Lower Eocene of Europe and India.

The upper division of the Libyan stage is typically developed in the valley of the Nile between Siut and Cairo; and on the right bank opposite Minieh it is found to consist of a succession of beds of limestone. It has also been described by Fraas (11) at one or two points between Cairo and Suez. The most characteristic fossils are Alveolinas of various species (*A. oblonga*, *A. frumentiformis*, etc.). *Fabularia Zitteli* is also abundant; and near Cairo the rock is in places nearly filled with the claws of *Callianassa*, from which circumstance Fraas bestowed upon the group the name of *Callianassa* bank. Nummulites are not so common as in the lower division.

An analysis of the fossils leads to the conclusion that the upper division of the Libyan is to be correlated with the Londinian (a part of the Lower Eocene), and Mayer-Eymar finds the nearest analogue in the sands of Cuise-la-Mothe.

The Mokattam stage occupies the greater part of the Eocene plateau north of the Baharia oasis; and near the valley of the Nile a tongue of it extends upon the top of the Libyan beds as far south as Siut. It also occurs over a considerable part of the plateau east of the Nile.

But it is in the hill of Mokattam near Cairo that it is best known. Owing to its accessibility this locality has been visited by many geologists, and several of them have published the results of their visits in more or less detail. The earliest satisfactory account is that of Fraas; while in more recent years we have descriptions by Zittel, Schweinfurth (24), Mayer-Eymar (17), and others. Schweinfurth's is by far the most complete and detailed account of the stratigraphy of the hill, and is accompanied by a map and sections in which he shows the distribution of the divisions which he has recognised. Mayer-Eymar has proved the

close correspondence between the Egyptian beds and those of France, and has attempted to correlate even the smaller sub-divisions.

The structure of the hill is very simple. It is formed of a series of beds dipping pretty regularly towards the east. Even at a distance two divisions are clearly recognisable, for the lower beds are white in colour and the upper beds brown. The lower division consists chiefly of fossiliferous limestones characterised by the abundance of nummulites, especially *N. Gizehensis* and *N. curvispira*. Amongst the other fossils we can mention only *Echinolampas Fraasi*, *E. africana*, and the remarkable crab *Lobocarcinus*. An interesting peculiarity of this division is the common occurrence of celestine (1, 4).

The upper division consists of an alternation of brown clays and sands, with beds of limestone. Fossils are not so numerous as in the lower beds, but some banks are filled with *Ostrea Clot-Beyi* and other species of oyster. The large nummulites have mostly disappeared, and *N. Gizehensis* is unknown; but *N. Beaumonti* and *N. sub-Beaumonti* occur throughout.

According to Zittel these divisions are only of local value and cannot be recognised, for example, at Beni Hassan and Minieh. The whole Mokattam stage must, therefore, be looked upon as one, and it is correlated by Zittel with the Parisian. Mayer-Eymar (17), however, recognises at Mokattam all the numerous sub-divisions which he has adopted in the Parisian of Europe, and he states that the resemblance to the European sequence is very close.

In the northern wall of the depression of Fayum a succession of beds very similar to that of Mokattam has been described by Schweinfurth (26). The most interesting of the fossils which he records are the remains of *Zeuglodon* and of a number of Teleostean fish which have been described by Dames. According to Schweinfurth the bed from which they are derived, corresponds with a part of the upper division of the Mokattam stage.

Throughout the greater part of Egypt the marine Eocene concludes with the Mokattam stage. But in the west,

near the oasis of Siwa, Zittel discovered nummulitic beds of a different character. They are only some 10 m. thick, and consist of white and brown limestones, made up almost entirely of fragments of shells. Amongst the fossils which occur in them are *Nummulitis intermedius*, and *N. Fichteli*, which belong to De la Harpe's seventh nummulitic horizon (including uppermost Eocene and lower Oligocene); but along with these are also found such typically Eocene forms as *N. Guettardi*, etc. The sea-urchins belong mostly to species which occur also in the Mokattam stage. The molluscs, according to Mayer-Eymar's determination, indicate a Bartonian age.

North of Mokattam rises the hill which is called Jebel Ahmar. It consists chiefly of a brownish sandstone rendered very tough by a siliceous cement. This is the Jebel Ahmar Sandstone, or Nicolia Sandstone of Zittel, and it rests upon the upper beds of the Mokattam stage. Silicified stems of trees are not uncommon in it, but other fossils are rare. Unger (30), however, has recorded the presence of a *Helix*, and of some almost indeterminable fresh-water shells. More recently Mayer-Eymar has discovered near its base a bed containing fresh-water shells. These he at first referred to the Upper Tongrian (17);[1] but on a closer examination he has found them to belong to the Lower Tongrian (18), and he looks upon the Jebel Ahmar Sandstone itself as Upper Tongrian. The usual view hitherto has been that the sandstone is Miocene; but the evidence for this was very weak.

Whatever the age of the Jebel Ahmar rock may be, it is generally agreed that it is the same as the sandstone which occupies so large a part of the desert west of Cairo. It extends from the Wadi Natrun southwards to about lat. 29° N., and a small area occurs also on the east side of the Nile. In some places the fossil trees are so abundant as to have suggested the name of "Petrified Forests". Along its southern margin it overlies a bed of oysters (*O. Fraasi* and *O. Cloti* [*O. Clot-Beyi*]) of Upper Mokattam age (16);

[1] The Tongrian is one of the divisions of the Oligocene.

but in the Wadi Natrun it is stated by Lyons to rest upon Miocene beds. The palæontological evidence, however, is not given, and these "Miocene" beds may well be Lower Tongrian.

The silicification of the Jebel Ahmar Sandstone and of the trees which it contains, is one of the most interesting problems of Egyptian geology, and has been frequently discussed. Schweinfurth was of opinion that it was due to the former presence of geysers or springs of heated waters, and the discovery of siliceous tubes penetrating the sandstone is strong evidence in favour of this view. Walther has given a figure (33, p. 476) showing these tubes standing up above the weathered surface of the rock. A similar kind of silicification on a large scale is taking place at the present day at the hot wells called Hammam-Meskhutin in the Algerian province of Constantine.

About a dozen miles west of the great pyramids Schweinfurth has found deposits which he referred to the Miocene (28). But they have since been examined by Mayer-Eymar (19), and according to him they belong to the Lower Tongrian. The fossiliferous beds are sometimes covered by a cap of dolerite.

Similarly the supposed Miocene beds on the high ground surrounding the depression of Fayum are believed by Mayer-Eymar to be of Ligurian and Tongrian age. But no satisfactory palæontological data have yet been published concerning these deposits.

Miocene.—The observations of Mayer-Eymar have thrown so much doubt on the supposed occurrences of Miocene rocks in the neighbourhood of Cairo that it has become necessary to exercise some caution in attributing a Miocene age to any of the Egyptian deposits. The difficulty is not lessened by the fact that geologists have used the term in very various senses.

We may, however, without much hesitation, accept as Miocene the rocks which form the walls and most of the floor of the oasis of Siwa (34), and which probably occupy most of the Barka plateau. In the oasis itself they consist of limestones and marls containing numerous echinoderms,

lamellibranchs, and other fossils. These beds are all marine; but south of the oasis, in places resting conformably upon the marine beds, are sandstones and fresh-water limestones with *Physa*, *Planorbis*, and *Limnaea*.

Marine beds containing a similar fauna occupy the greater part of the valleys between Jebel Geneffeh and Jebel Attaka, and between Jebel Attaka and the Northern Galala (5). Fuchs, after a comparison of the fossils from Jebel Geneffeh and from Siwa, has come to the conclusion that they are of an age intermediate between the first and second Mediterranean stages.

Pliocene and recent.—Since the Miocene period the Egyptian area seems to have been subject to comparatively little disturbance; but there has been a gradual elevation of the whole region. At the foot of the Mokattam Hill at Cairo marine sands are found banked up against a vertical cliff of Eocene rocks, which is itself bored by Lithodomus or similar shells (11, 9, etc.). The upper limit of the borings is some 200 feet above the level of the Mediterranean, indicating a rise of land of at least that extent.

Near the pyramids of Giza there is a sand which is characterised especially by the well-known fossil *Clypeaster ægyptiacus* or *pliocenicus*. In the Wadi el Mellaha, some four miles south of the great pyramids there is another bank of sand and sandstone, containing a number of shells of the genera *Strombus*, *Cassis*, *Conus*, etc. According to Mayer-Eymar it is clear that both these deposits lie on the same geological horizon, and from the character of the fauna in the latter he refers them both to the "diluvial" period (17). Neumayr, however, has since shown that they are Pliocene (20).

Still more recent probably than these are the deposits which form the isthmus of Suez. They have been described by Fuchs (12), who comes to the conclusion that, in spite of the great difference between the faunas of the Red Sea and of the Mediterranean, there was a connection between the two seas until at least Pliocene times. He finds nothing but Pliocene or later deposits between Port Said and Suez. Recent deposits with a Mediterranean fauna extend south-

wards beyond the Balah Lakes; from here to Jebel Geneffeh and the middle of the Bitter Lakes, we have recent fluviatile deposits of the Nile; while between these and Suez are recent deposits of the Red Sea. Immediately surrounding the Bitter Lakes there are recent deposits containing an impoverished Red Sea fauna.

Fraas (11) states that Miocene beds are developed at Shaluf (which lies on the Suez Canal some four miles south of the Little Bitter Lake); and this might be supposed to be the remains of an isthmus already existing in Pliocene times. But Fuchs definitely asserts that there is no Miocene present there, or anywhere else along the course of the Canal.

Intrusive and volcanic rocks.—Intrusive dykes of diorite and similar rock appear to be common in the crystalline rocks; but among the sedimentary deposits eruptive rocks of any kind are very rare. Lyons mentions a mass of olivine dolerite intruded into the Nubian Sandstone at Jebel Burka, about 20 m. W.N.W. of Wadi Halfa (16). In the oasis of Baharia a basaltic rock breaks through the Cretaceous Sandstone and forms the caps of some of the hills; but what its relations are to the nummulitic rocks does not appear. It has been examined by Zirkel and found to be an olivine basalt (34). A very similar rock, also an olivine basalt or dolerite, has been described by Arzruni (2) from Abu Zabel on the Ismailia Canal, where it is erupted through Tertiary strata.

In the Wadi Araba a number of small intrusions of basaltic rock have been discovered by Schweinfurth and Walther (32).

Lastly Mayer-Eymar (19) has found a bank of dolerite resting upon Lower Tongrian beds in the hills of the Kum-el-Chachaf. He believes these and the igneous rocks of Baharia and Abu Zabel to have been erupted at the same time, that is to say, at the close of the Lower Tongrian period. If this be so, it is not unlikely that the hot springs which caused the silicification of the Jebel Ahmar Sandstone and of the Petrified Forests, represent the close of the only period of vulcanicity which there has been in Egypt since, perhaps, Archæan times.

Note on "The Geology of the Sahara".—Since the article on the Sahara was written, the fossils collected by Overweg near Bir-el-Hassi, and described by Beyrich, have been subjected to a second critical examination by Frech.[1] The general conclusion that the beds from which they were derived are Devonian remains unaltered.

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PHILIP LAKE.

THE EVOLUTION OF THE THERMOMETER.

THE earliest thermometer appears to have been made towards the close of the sixteenth century, but the name of the actual discoverer is not known. The invention has been variously attributed to Robert Fludd, a London physician and a prolific writer on medicine and alchemy, who died in 1637; to Galileo (prior to 1597); to Cornelius Drebbel of Alcmar (see Boerhaave's *Chemistry*, translated by Shaw, i., 152); to Sanctorius of Padua, and to Father Paul of Cracow, who died about 1591. The balance of evidence appears to rest with Sanctorius, who, in his *Commentaria in Artem Medicinalem Galeni* (p. 736, 1613), claims to have discovered the thermometer. John Alphonsus Borelli (*De Motu Animalium*, ii., 175, 1680), and Malpighi (*Posthumous Works*, Amsterdam, 30, 1697), also give him credit for the invention.

All the earlier thermometers depended upon the expansion of air enclosed in a bulb on the end of a tube, which terminated in an open-topped bulb containing a coloured liquid, or dipped into an open vessel holding the liquid. The expansion or contraction of the air was thus indicated by the movement of the liquid within the tube, but as the change of volume of the air was affected by the atmospheric pressure, and the tube was either not graduated or only marked out in divisions as the taste of the maker dictated, the thermometers were of but little value.

The first alcohol thermometer is said to have been made by Galileo in 1611 or 1612, and the "Florentine thermometers" containing spirit of wine in a sealed tube, were employed about the middle of the seventeenth century, by the Academists del Cimento, and were introduced into England by Boyle. In these thermometers, no attempt appears to have been made to expel air before sealing them, and a lucky find of a number of them in 1829, has shown that their graduation was purely arbitrary. The divisions were duodecimal, each consisting of an enamel bead sealed upon

the tube, and every twelfth division was marked by the use of a larger bead or one of a different colour. Further information as to these thermometers, some of which were sent to the Queen of Poland by the Grand Duke of Tuscany in 1657, and thence went to Paris, has recently been published by M. Maze (*Comptes Rendus*, vol. cxx., p. 732, and vol. cxxi., p. 230).

The difficulty of fixing the zero and the distance between the divisions of the thermometer was early recognised, and the want of uniformity was probably due to the fact that chemists were not aware that the freezing and boiling points of liquids were constant. Hooke, however, in 1665, described in his *Micrographia*, p. 38, a spirit thermometer having the zero at the freezing point of water, which he apparently recognised as constant. Boyle also proposed this standard, recommending the use of distilled water, as he was persuaded that waters of different origin congealed at different temperatures (see *Boyle's Works*, abridged by Shaw, vol. i., p. 579). As an alternative, he proposed the congealing point of oil of aniseed as the zero.

In 1692 Mr. Edmund Halley, Secretary of the Royal Society (see *Phil. Trans.*, 1692-3, vol. xvii., p. 650), published an important paper on the construction of thermometers "for use at any place without adjusting by a standard". He stated that he was not aware of any thermometers which had been graduated except as the workman's fancy dictated, although they were usually graduated from a fixed point such as the freezing point of water, or "the point where the spirit stands when it is so cold as to freeze oil of aniseed". After discussing the relative merits of various liquids, and mentioning that mercury might be most properly used were its expansion more considerable—probably the first mention of mercury thermometers—he recommended spirit of wine as the best.

In 1694 Professor Renaldini of Padua suggested the fixation of unity by the temperature of a mixture of eleven parts of cold water with one part of boiling water, the second degree being obtained by mixing ten parts of cold with two parts of boiling water, and so on. In 1701 New-

ton introduced his linseed-oil thermometer, taking the zero as the temperature of thawing snow, and 12° as that of the blood of a living animal. In a "translation from the Latin"—apparently of a paper by Newton—in the *Philosophical Transactions* for 1701, vol. xxii., p. 824, there is a "table of the degrees of heat," the lower temperatures being presumably taken with the oil thermometer, and the higher by comparison with heated iron. In this table, 0° is the temperature of freezing water, 12° blood heat, and 34° that of strongly boiling water; 192° was the heat of live coals burning in a kitchen fire without the aid of bellows.

A thermometer which is said to have drawn Fahrenheit's attention to the subject of the graduation of thermometers, is said to have been described by M. Amontons in the *Mémoires de l'Academie Royale de Paris* in 1701 or 1702, although the writer has been unable to find the paper in these *Mémoires*. In Amontons' thermometer, by which he is frequently credited with having first shown that the boiling point of water is constant, a long glass tube was turned upwards at the bottom and terminated there in a bulb. The bulb contained air, and the tube contained mercury which formed the indicator. The tube was graduated—on the assumption that the atmospheric pressure is equal to a pressure of twenty-eight inches of mercury—in inches and parts of an inch. The highest point, forty-five inches above the level of the mercury in the bulb, was numbered seventy-three, *i.e.*, the height above the mercury plus the atmospheric pressure. His thermometer was thus extremely long and cumbrous, and was inaccurate owing to no allowance being made for variation of atmospheric pressure.

The Fahrenheit thermometer was first graduated with a scale of 180°, the zero being described as "temperate" and corresponding with 9° of the present centigrade thermometer, 90° being "blood heat," and - 90° being the temperature of a mixture of ice and salt, supposed to be the greatest degree of cold obtainable. In 1714, at the suggestion of Claus Roemer, Fahrenheit changed his zero to the

lowest of these points, taking the freezing point of water as 8° and blood heat as 24°, and finally, about 1720, he divided the degrees into fourths, the freezing point of water then standing at 32° F., blood heat at 96°, and the boiling point of water at 212°. The latter improvement was accompanied by the replacement of spirit of wine by mercury (see *Phil. Trans.*, vol. xxxiii., p. 1, 1724). Although this appears to have been the first true mercury thermometer, the use of that material had been proposed as early as 1692 by Halley, as already mentioned.

In 1724 M. de l'Isle of St. Petersburg introduced a system of graduation in which the boiling point of water formed the zero, and the temperature of the cellars of the Paris Observatory 100°. In 1733 the same observer presented a paper to the Academy of Sciences of St. Petersburg (see also *Phil. Trans.*, vol. xxxix., p. 221, 1736), on a mercurial thermometer, having its zero at the boiling point of water and marked in degrees, each of which represented $\frac{1}{10000}$ part of the total volume of mercury contained in the thermometer.

The original thermometer of René de Reaumur of Rochelle, although far from delicate, was soon largely used. It was introduced before 1730 (see *Mémoires de l'Academie Royale de Paris*, p. 645, 1730), and had a large bulb—as much as three or four inches in diameter—containing a known volume of spirit, the tube being so graduated that each degree represented $\frac{1}{1000}$ part of the total spirit. Ultimately, the system of graduation was altered so that the freezing point of water formed the zero and the boiling point was marked at 80°, the bulb being also reduced in size, but the most important improvement due to Reaumur appears to have been the careful expulsion of air from the spirit and from the thermometer tube before sealing, a precaution which had not previously been taken except in the most primitive way.

In 1740 M. du Crest introduced a spirit thermometer provided with the scales of Reaumur, Fahrenheit, and De l'Isle, and in 1742 the valuable improvement of Anders Celsius was introduced. This thermometer, which was formerly known

as the Swedish and afterwards as the centigrade thermometer, and was not used to any extent in France until after the Revolution, was divided centesimally. The boiling point of water was at first marked as 0° and the freezing point as 100°, but, at the suggestion of Linnæus, this graduation was reversed, the temperature of melting ice being taken as zero and that of boiling water as 100°.

Towards the middle of the seventeenth century, attention began to be directed to methods for measuring higher temperatures than can be shown by ordinary thermometers, and a paper in the *Philosophical Transactions* for 1747 (vol. xl., pp. 397 to 408) stated that Dr. Muschenbroek of Utrecht had invented a "metalline" thermometer, in which the expansion or contraction of rods of metal was caused to show changes of temperature. The instrument, as improved by Dr. Desaguliers, has been described in his *Experimental Philosophy* (vol. i., p. 421, 1734).

The next important improvement in this branch of thermometry was the introduction of the Wedgwood pyrometer (*Phil. Trans.*, vol. lxxii., p. 305, 1782; and vol. lxxiv., p. 358, 1784), in which the high temperatures of a furnace were measured by the diminution in bulk of a block prepared from a pure fire-clay under constant conditions as to the amount of moisture used, and the pressure applied in moulding.

Thermometers for *registering* the highest and lowest temperatures attained during the absence of the operator, were introduced as soon as the ordinary thermometer attained to the dignity of an instrument of precision, and although want of space forbids a description of them, the following brief account, with the sources from which it has been derived, may be of use to those who wish to gain information as to the earlier history of this form of thermometer. J. H. van Swinden (*Dissert. sur la Comp. der Thermom.* (?), p. 253, 1770) described, about 1770, maximum and minimum thermometers made on a plan first contributed by Bernouilli to Leibnitz, and Lord Cavendish, in 1757, described somewhat similar instruments of his own invention (*Phil. Trans.*, vol. l., p. 300). In 1782 the well-

known Six's maximum and minimum thermometers were described (*Phil. Trans.*, vol. lxxii., p. 72), and in 1790 and 1794 respectively, the maximum and minimum thermometers of Rutherford were introduced (*Trans. Roy. Soc. Ed.*, vol. iii., p. 247, 1794). In 1826 Blackadder (*Trans. Roy. Soc. Ed.*, vol. x., p. 337) introduced a registering thermometer in which clockwork was used. It may be mentioned that a seventeenth century thermometer, somewhat resembling that of Six, is in existence among the instruments of the Florentine Academy.

So much for the history of the thermometer as far as the commencement of the present century. Many of the works quoted are not readily accessible, but those bearing on the improvements which have since been introduced may be easily found in our scientific libraries, and the reader may follow the process of evolution from the imperfect instruments before described, to the Baly and Chorley thermometer in which mercury is replaced by a fusible alloy of potassium and sodium for determining temperatures above the boiling point of mercury, and the Le Chatelier pyrometer and the modification of it introduced by Professor Roberts-Austen for the determination of the high temperatures reached in furnaces.

George T. Holloway.

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EMANCIPATION FROM SCIENTIFIC MATERIALISM.[1]

IT has at all periods been a source of complaint that so little unanimity should prevail with regard to the most important and fundamental of human problems. In our own times, however, the grievance concerning one of the greatest of these questions has almost disappeared. For, although there still exist many and varied contradictions, nevertheless it may be asserted that scarcely in any age has there been so comparatively close an agreement regarding our conception of the outer world of phenomena as exists in this present scientific century of ours. From mathematician to practising doctor, every scientifically thinking man, if called upon to express his opinion as to the "inner structure" of the universe, would sum up his ideas in the conception that things consisted of atoms in motion, and that these atoms and their mutual forces were the final realities underlying all phenomena. We read and hear, with countless repetition, the statement that the only intelligent explanation of the physical world is to be found in a "Mechanics of the Atoms"; matter and motion appear as the final principles to which natural phenomena in all

[1] A paper read at the third general sitting of the Assembly of the Society of German Scientists and Physicians at Lübeck, 20th September, 1895. By Wilhelm Ostwald, Professor in Chemistry at the University of Leipzig. Authorised translation by F. G. Donnan and F. B. Kenrick.

their variety must be referred. This conception we may term scientific materialism.

I here propose to state my conviction that this so generally accepted view is untenable; that this mechanical idea of the universe does not fulfil the purpose for which it was designed, and that it is inconsistent with undoubted and generally known and recognised truths. The conclusion to be drawn from this is obvious. The scientifically untenable view must be abandoned and its place filled, if possible, by a new and a better one. The natural question as to whether such another and better conception can be found, I think I can answer in the affirmative. My remarks consequently divide themselves naturally into two parts: a destructive part and a constructive part. As in all cases, so here, destruction is easier than construction; the inefficiency of the customary mechanical treatment is more easy to demonstrate than the efficiency of the new one, which I would characterise as the "energetical" view. I may remark at once that the new view has already had an opportunity of proving its worth on quiet reflection, and by impartial research in particularly favourable regions of experimental science. Although this cannot prove its correctness, it still gives the new conception a claim to notice.

It will not perhaps be superfluous to state at the outset that I am dealing to-day exclusively with a question of natural science. I draw aside on principle from all conclusions of an ethical or religious nature which may be deduced from the result of this discussion. I do this not because I undervalue the significance of such conclusions, but because my arguments have been founded, independently of such considerations, on the firm ground of the exact sciences. For the tillage of this soil also, it may be said that he who putteth his hand to the plough and looketh back is not fit for this kingdom. The scientific investigator of nature is bound to set forth his results neither to pain nor to please, and we may trust ourselves to the power of Truth which, though perhaps temporarily, can never lead far from the right way him who seeks her earnestly.

I do not fail to recognise that my undertaking places me in opposition to the views of men who have done great things for science, and to whom we all look up in wonder and admiration. But do not charge me with presumption if I oppose myself to such men in a matter of so great importance. You do not call it presumption when the sailor on duty at the mast-head turns the great ship from her course by his cry of "Breakers ahead!" albeit he is but an insignificant member of her crew. His duty is to announce what he sees, and did he fail to do this he would prove untrue to this duty. It is in this sense a duty which I am to-day discharging. Nevertheless, no one is bound to alter his scientific course in answer to my cry of "Breakers ahead!" Every one is at liberty to test whether it is a reality that stands before my eyes, or whether a mirage deceives my vision. I believe however that the special nature of my scientific duties gives me for the moment a clearer insight into certain phenomena than may be had from other points of view, and for this reason I could not but regard it as wrong, were I for extraneous motives to leave unsaid what I have seen.

In order to find our way clearly through the infinite variety of the world of phenomena, we always make use of the same scientific method, namely, grouping together things that are similar and in variety seeking the non-varying. In this way our gradual mastery over the infinitely various phenomena of the outer world is acquired and ever more effective means of co-ordination are continually developed. From the simple *list* we proceed to the *system*, from this again to the *law of nature*, and the most comprehensive form of the latter condenses into a *general conception*. We perceive that the phenomena of the actual world, limitless as is their variety, form nevertheless only certain perfectly definite and particular cases of the theoretically conceivable possibilities. The significance of a Law of Nature consists in the determination of the *actual* among the *possible* cases, and the form to which each can be referred is the finding of an *invariant*, that is to say, a quantity which remains

unchanged even when all the other determining elements vary within the possible limits imposed by the law. Thus we perceive that the historical development of scientific conceptions is ever associated with the discovery and working out of such invariants; in them we behold the mile-stones which mark the track traversed by human knowledge.

Such an invariant of universal significance was found in the idea of mass. This gives not only the constants of the laws of Astronomy, but also appears no less invariable in the case of the most deep-going changes to which we can subject the objects of the outer world, namely, chemical changes. Accordingly this idea appeared to be excellently suited to form the central point of scientific law. Of course it was too poor in connotation to serve alone for the representation of the various phenomena, and had therefore to be correspondingly extended. This was effected by fusing with the simple mechanical idea the series of properties which are associated with the property of mass and are proportional to it. In this manner arose the conception of matter, in which everything was summed up that was associated in our sense-impressions with mass, and which always accompanied it, as, for example, weight, extension in space, chemical properties, etc., and the *physical law* of the conservation of mass passed into the *metaphysical axiom* of the conservation of matter.

It is important to observe that, with this extension, a great many hypothetical elements crept into a conception originally free from hypothesis. For example, in the light of this conception a chemical reaction had, contrary to appearance, to be so considered that none of the matter affected by the chemical change could possibly disappear and be replaced by new matter with new properties. On the contrary, the view required the assumption that when, for instance, all the perceptible properties of iron and oxygen had disappeared in iron oxide, nevertheless iron and oxygen still existed in the body produced and had only assumed other properties. At present we are so accustomed to such an idea that it is difficult for us to conceive its

strangeness, indeed its absurdity. But if we consider for a moment that all we know of a given substance consists in the knowledge of its properties it is evident that the assertion that a certain substance continues to exist without possessing these properties is not very far removed from pure nonsense. As a matter of fact, this purely formal assumption serves simply to unite the general facts of chemistry, in particular the stöchiometric laws of mass, with the arbitrary notion of an intrinsically unvarying matter.

But even the so extended idea of matter, together with the necessary attendant suppositions, is not sufficient to embrace all phenomena, not even indeed in the inorganic world. We think of matter as something in itself intrinsically motionless and unvarying; hence in order to make the representation of an ever-changing world possible, we must supplement this conception with another independent one which gives expression to this changeableness. A conception of this sort was, in the most successful manner, developed by Galileo, the founder of scientific physics, in the idea of force as the constant cause of motion. Galileo had, in fact, discovered an invariant of great importance for the varying phenomena of free and indirect fall; and with the assumption of the intrinsically constant force of gravitation, whose effects continuously accumulate, the complete representation of these phenomena became possible. The significance of this conception became evident when Newton, with his idea that the same force acted between the heavenly bodies, brought the whole star-world under the sway of science. It was this step forward in particular which gave rise to the conviction that just as the astronomical so also all other physical phenomena must be capable of representation by this means. When, moreover, at the beginning of our century the efforts of numerous, and especially French, astronomers had shown that Newton's law of gravitation could not only represent the motions of the heavenly bodies to a first approximation, but withstood also the far severer test of a second approximation in their ability to express with equal certainty and accuracy the

small deviations from the typical motions (*i.e.*, the perturbations), the confidence in the sufficiency of this theory must have been extraordinarily strengthened. What was then more likely than the expectation that the theory which had shown itself so perfectly adapted to represent the motions of the great bodies of the Universe, must be the correct, nay, the only means of bringing the events of the little world of atoms under the sway of science? So arose the mechanical conception of nature according to which all phenomena (at first of inorganic nature) were to be finally referred simply to the motions of atoms according to the same laws which had been recognised to hold for heavenly bodies. That this conception should be immediately carried over from the region of inorganic to that of animated nature, was a necessary consequence when it was once perceived that the same laws which hold in the former, claim here also their inviolable rights. This conception of the universe found its classical expression in Laplace's idea of the "Universe-Formula" by means of which every past and future event was to be capable of deduction by rigid analysis applied to mechanical laws. For this purpose an intellect was to be required which though far beyond the human mind in power was not essentially of a different nature.

We do not generally notice in what an extraordinary degree this widespread view is hypothetical, nay, metaphysical; on the contrary we usually regard it as the most exact expression for the actual relations. Nevertheless it must be remarked that a confirmation of the natural deduction from this theory, namely, that all non-mechanical phenomena such as heat, radiation, electricity, magnetism and chemical action are actually mechanical, has in no single case been obtained. In no single case has the attempt to represent the actual relations by means of a mechanical system so far succeeded that nothing remained over to explain. I grant that for many individual phenomena, the mechanical analogues have been given with more or less success. But all attempts to completely represent the whole of the known facts in any department by means of some such mechanical analogue have resulted without

exception in some unexplainable contradiction between what really happens, and what we should expect from our mechanical model. This contradiction may long remain hidden ; but the history of science teaches us that it sooner or later makes its inevitable appearance, and that all we can say with complete certainty regarding such mechanical similes or analogues—usually termed mechanical theories of the phenomena in question—is that they will doubtless on some occasion fail.

The history of optical theories offers a striking example of these facts. So long as the whole of optics embraced only reflection and refraction, its phenomena could be represented by the mechanical system proposed by Newton, according to which light consisted of small particles which were shot out in straight lines from the radiating object, and obeyed the laws of motion for perfectly elastic bodies. The fact that another mechanical view, the vibration theory of Huygens and Euler, yielded just as much in these respects might well have awakened doubts as to the exclusive ability of the earlier hypothesis to meet the requirements of the case, but it was not able to usurp the latter's scientific position. When, however, the phenomena of interference and polarisation were discovered, Newton's mechanical analogue showed itself to be quite unsuitable, and the other, namely, the *vibration theory*, was considered established, since from its assumptions the chief points at least of the new phenomena could be deduced.

The life of the vibration theory as a *mechanical* theory has also had its bounds, for in our own time it too has been carried to its grave without drum or fife, and its place taken by the electro-magnetic theory of light. A post-mortem examination reveals clearly the cause of death ; it has resulted from the failure of the mechanical parts. The hypothetical ether whose task it was to "beat" had to fulfil its duty under particularly difficult conditions. For the phenomena of polarisation demanded unconditionally that the vibrations should be transversal. Now this presupposes a solid body, and the calculations of Lord Kelvin have finally shown that a medium with such properties as

were thus required of the ether could not possibly be stable, and, as a necessary consequence, can have no physical existence. Doubtless the idea of sparing the now generally received electro-magnetic theory of light a similar fate prompted the immortal Hertz, to whom this theory owes so much, when he expressly declined to see in it anything beyond a system of six differential equations. That the evolution of the theory should end in this point is a far more convincing argument than any I could adduce against the permanent value of the theoretical methods previously followed on mechanical lines.

But you may urge the fruitfulness of these theories. Yes, they were fruitful in so far as they contained correct elements, just as they were harmful on account of the false ones. Which were the right and which were the wrong elements, however, was only revealed after long and dearly bought experience.

The result of our remarks up to the present is in the first place purely negative; we have learned how *not* to proceed, and it may appear of little use to bring forward such negative results. However we may already note one gain here which many of you will not consider worthless. In this way we discover the possibility of critically correcting a view which in its own time created no small sensation, and caused many of those interested great trouble. I refer to the widely known views first expounded twenty-three years ago by the celebrated Berlin Professor of Physiology, Emil du Bois-Reymond, at the Leipzig meeting of the German Association of Natural Philosophers, and later in some other much-read papers; views dealing with the prospects of our future knowledge of nature and culminating in the much-discussed *ignorabimus*. In the long controversy which this speech gave rise to du Bois-Reymond has, so far as I can see, victoriously withstood all attacks, and naturally so, since all his opponents have proceeded from the same premises which led him to his *ignorabimus*, and his conclusions stand as firm as the basis upon which he built them. This basis, which meanwhile had been called in question by no one, is the me-

chanical conception of the universe, namely, the assumption that the resolving of phenomena into a system of moving particles is the goal to be aimed at in our explanation of nature. Should, however, this foundation fall—and we have seen that it must—then with it goes the *ignorabimus*, and science is once more free to move onward.

I do not believe that this result will be received with astonishment, for judging by my own experience, no investigator of nature has seriously believed in the *ignorabimus*, although it was not clear where the weak point of the argument lay. Hence the result gained by the rejection of the mechanical conception of the Universe, namely, the banishment of that menacing spectre, may well be of some value to many a thinker unable to find a flaw in the resistless logic of du Bois-Reymond's argument.

What has here been set forth for the sake of clearness with respect to special discussions such as the foregoing, has, however, a far wider significance. The doing away with the mechanical construction of the universe goes down to the very foundations of the whole materialistic conception of things, taking the word materialistic in its scientific sense. If it appears a vain undertaking, ending with every serious attempt in final failure, to give a mechanical representation of the known phenomena of physics, we are driven to the conclusion that similar attempts in the incomparably more complicated phenomena of organic life will be still less likely to succeed. The same fundamental contradictions occur here also, and the assertion that all the phenomena of nature can be primarily referred to mechanical ones cannot even be designated here as a practical working-hypothesis ; it is simply incorrect.

This error appears more clearly when viewed in the light of the following fact. The equations of mechanics all possess the property that they still hold good when the sign of the quantity denoting *time* is changed. That is to say, theoretically perfect mechanical processes can take place just as well backwards as forwards. In a purely mechanical world there would be, therefore, no Before and

no After, in the sense of our world; the tree could return again to the sapling and the sapling to the seed, the butterfly transform itself once more into the caterpillar, and the old man become again a babe. For the fact that this does not occur, the mechanical conception of the world has no explanation to offer, and can have none on account of the already mentioned property of mechanical equations. The evident irreversibility of actual natural phenomena proves, therefore, the existence of processes which cannot be represented by mechanical equations, and with this statement the judgment on scientific materialism is passed.

We must accordingly, and this appears to follow with absolute certainty from these considerations, give up all hope of getting a clear idea of the physical world by referring phenomena to an atomistic mechanics. But, perhaps one of you will say, what means shall we have left of picturing to ourselves what really occurs in nature when the conception of atoms in motion is abolished? To such a question I would answer: Thou shalt not make unto thyself any image or likeness. Our task is not to view the world in a more or less bedimmed and crooked mirror, but as directly as the nature of our minds will permit. To co-ordinate realities, *i.e.*, definite and measurable quantities, so that when certain of them are given the others can be deduced, is the problem set before science, and this problem cannot be solved by assuming as substratum any hypothetical analogue, but only by the determination of the mutual relations existing between measurable magnitudes.

Undoubtedly this way is long and tiring, yet it is the only permissible one. But we need not tread this path with bitter self-renunciation hoping that it will finally lead our grandchildren to the longed-for summit. No, it is we who are the fortunate ones, and the most hopeful bequest which the departing century can bestow on the one that is just dawning is the replacement of the mechanical by the "energetical" conception of the universe.

I consider it of the greatest importance to state here that all this is by no means a novelty, a production of to-day. No, for half a century it has, though unrecognised,

been in our possession. Here indeed if anywhere we may fitly apply the words: "Geheimnissvoll offenbar" (mysteriously revealed). Daily could we read it and we understood it not.

When Julius Robert Mayer fifty-three years ago first discovered the equivalency of the various natural forces, or as we now say, the various forms of energy, he had already taken an important step in the critical direction. But according to an ever-recurring law in collective thinking, a new idea is never accepted in the pure and unsullied form in which it is offered. He who has not inwardly experienced its development, but who has received the knowledge from without, seeks above all to adapt that which is new as well as possible to his previous notions. In this way the new idea is marred, and, even if not actually perverted, nevertheless robbed of its best power. Indeed so active is this peculiarity of thought that it does not even leave free the discoverer himself. Copernicus' powerful intellect sufficed indeed to cause sun and earth to change places in their motions, but failed to conceive the motions of the other planets in their simplicity; for these he retained the traditional theory of the epicycles. We see the same thing in Mayer's case. The task of the succeeding generation consisted, as is almost always the case, not simply in reaping the results of the new doctrine, but rather in separating piece by piece the arbitrary and extraneous additions until finally the fundamental idea should appear again in its pure simplicity.

We observe also in our case a similar development. When J. R. Mayer had set forth the law of equivalency his idea of equivalent transmutability of the different forms of energy was in its simplicity too strange to be directly accepted. Indeed the three scientists to whom we are mostly indebted for the working out of this law—Helmholtz, William Thomson and Clausius—all three believed that the law could be "explained" by assuming that the different forms of energy were fundamentally the same, namely, *mechanical* in nature. In this way what appeared to be the most urgent need was satisfied, namely, a direct

connection with the dominating mechanical conception of nature. But a fundamental part of the new thought was thereby lost.

Half a century has been necessary to mature the conviction that this hypothetical addition to the law of energy is in reality no deepening of insight but a renunciation of its most important aspect: its freedom from every arbitrary hypothesis. It was not, however, the recognition of this general fact which actually brought about this advance, namely, the rejection (as far as it has gone) of the mechanical explanation, but rather the final failure of all attempts to interpret satisfactorily by the mechanical treatment the phenomena connected with the remaining forms of energy.

But you will be impatient to learn how it is to be possible by such an abstract idea as energy to form a conception of the universe which shall be comparable, in clearness and intuitiveness, with the mechanical one. The answer ought not to be difficult. What do we know of the physical world? Obviously only what is vouchsafed us through our organs of sense. But under what conditions are these organs set in action? Turn the matter as we may, the only principle we find common to all is this: *The Organs of perception react in response to differences of energy between them and the surroundings.* In a world in which the temperature was everywhere that of our bodies we should have no experience of heat, just as [illegible] have no perception of the constant atmospherical pressure under which we live. Not till we have produced a space of different pressure do we become aware of its existence.

Well and good, this you will be ready to admit. But you will not be so ready to abandon matter, for energy must of course have a *carrier*. But I ask in return: Why? Since our total knowledge of the outer world consists in its energy relations, what right have we to assume in this very outer world the existence of something of which we have had no experience? But energy, it has been urged, is only something thought of, an abstraction, while matter is a reality; exactly the reverse, I reply. Matter is a thing

of thought which we have constructed for ourselves (rather imperfectly) in order to express that which is lasting in the changeableness of phenomena. Now that we begin to grasp that the Actual, *i.e.*, that which *acts* upon us, is energy alone, we must inquire in what relation the two conceptions stand to one another, and the result is undoubtedly that the predicate of reality can be affirmed of Energy only.

This decisive aspect of the new conception will perhaps stand out more clearly if I sketch the development of the idea in question in a short historical *resumé*. We have already seen that progress in science is characterised by the discovery of ever more general invariants, and I have also indicated how the first of these invariable quantities, mass, was extended to matter, *i.e.*, mass accompanied by volume, weight, and chemical properties. This conception was, however, obviously insufficient to embrace phenomena in their continual changeableness, and from Galileo's time onwards the idea of force has been added in order to suit this phase of nature. But force lacked the property of constancy, and after *vis viva* and work had been found to be mechanical functions exhibiting the properties of partial invariants, Mayer discovered in energy the most general invariant which rules the whole dominion of physical forces.

In accordance with this historical development, matter and energy continued to exist side by side, and all that could be said about their mutual relation was that they occurred for the most part together or that matter was the carrier or holder of energy.

But are matter and energy things essentially different from one another, as perhaps Body and Soul? Or is not rather all that we know and say of matter already contained in the idea of energy, so that we can represent with this latter quantity the totality of phenomena? According to my conviction there can be no doubt as to the answer. Hidden in the conception of matter are the following ideas: first of all, mass, that is, the capacity factor of kinetic energy; further, the occupation of space or volume energy; again, weight or that particular kind of distance energy

which appears as gravitation; and finally chemical properties, *i.e.*, chemical energy. It is always a question of energy alone, and if we imagine the various kinds of energy removed from matter there remains *nothing*, not even the space it occupied; for space makes itself known only through the expenditure of energy which the penetration into it requires. Matter is therefore nothing but a group of various forms of energy co-ordinated in space, and all that we try to say of matter is really said of these energies.

What I am endeavouring to lay down is so important that you will pardon my venturing to approach the subject from another quarter. Allow me to use for this purpose the most drastic illustration I can find. Imagine that you receive a blow from a stick. What do you feel, the stick or its energy?

The only possible answer is: The energy. For the stick is the most harmless thing in the world as long as it is not wielded. But we can also knock ourselves against a stationary stick. Certainly. What we perceive, as already stated, are *differences* of energy conditions relative to our sense organs, and it is consequently immaterial whether the stick moves towards us or we towards the stick. If both we and the stick are moving in the same direction with the same velocity, the latter has no further existence for our sense of feeling, for it can no longer come in contact with us and effect an exchange of energy.

These considerations show, I hope, that *all* that we have until now been able to express by the ideas of Matter and Force—and indeed much more besides—may actually be expressed by the idea of energy. It is a question simply of transferring to this conception those properties and laws which were formerly ascribed to matter and force. We gain further the enormous advantage of doing away with the contradictions which were attendant on the former method of treatment, and to which I alluded in the earlier part of my paper. By making no assumption as to the relation between the different forms of energy, except that given by the law of conservation, we leave ourselves

free to study objectively the various properties of these forms of energy. We can, further, by rational consideration and arrangement of these properties, establish a system which shall represent explicitly not only the similarities, but also the differences in these forms of energy, and which will therefore lead us much further scientifically than would be possible when slurring over their differences through the hypothetical assumption of their "inner" identity. A good illustration of the meaning I intend to convey may be found in the kinetic hypothesis of gases which at present enjoys almost universal recognition. According to this hypothesis the pressure of a gas arises from the blows delivered by the moving particles. Now pressure is a quantity which possesses no special direction : a gas exerts pressure in all directions equally ; but a blow is caused by a moving object, and this motion has a definite direction. Consequently one of these quantities cannot be referred directly to the other. The kinetic hypothesis gets round this difficulty by the assumption that the blows occur uniformly in all directions, whereby the vector-property really possessed by the blow is artificially done away with. In this case the artificial adaptation of the properties of the different energies is successful, but in other cases it is not completely possible. The factors of electrical energy, potential and electrical quantity, form a case in point—quantities which I propose to call *polar*, *i.e.*, they are characterised not only by a numerical value but also by a sign in such a way that the sum of two equal values with opposite signs is equal to zero and not to their double value. In mechanics the purely polar quantities are unknown, and this is why all attempts to set up even a partially workable mechanical hypothesis for electrical phenomena must essentially fail. Should we succeed in contriving a mechanical magnitude with polar properties (as is perhaps not impossible, and certainly worthy of careful consideration) we should have the means wherewith to picture to ourselves mechanically at least a few phases of electrical phenomena. We may say with certainty that it will be a question of a few only, and that the imper-

fection of all mechanical hypotheses will show itself here again and will prevent the complete carrying out of the idea.

But even if the laws of natural phenomena may really be reduced to the laws of the corresponding forms of energy, what advantage do we gain therefrom? First, the very considerable one, that a science free from hypothesis becomes possible. We seek no longer forces whose existence we cannot prove between atoms which we cannot observe, but we judge a process by the kind and quantity of the vanishing and appearing energies. *These* we are able to measure, and all that it is necessary to know may be expressed in this manner. What an enormous general advantage this is, will be clear to every one whose scientific conscience has suffered under the continual amalgamation of facts and hypothesis which the physics and chemistry of to-day offer us as rational science. Energetics offers us a means of fulfilling in its true sense the demand of Kirchhoff so oft misunderstood, namely, the substitution of the description of phenomena for the so-called explanation of nature. With this freedom from hypothesis appears a methodical unity which, it may be unhesitatingly affirmed, has up till now never been attained. I have already referred to the philosophical significance of this unity of principle in the conception of natural phenomena; although obvious from the very nature of the case, I may still call attention in particular to the enormous advantage with regard to the teaching and understanding of science which accrues from this philosophical unification. To illustrate by an example: we may assert that all equations without exception which connect two or more different classes of phenomena must necessarily be equations between energy values, others are altogether impossible. This follows as a necessary result from the fact that besides the intuitive ideas of space and time, energy is the only quantity which is common to each and every class of phenomena; and therefore energy quantities are the only ones which can possibly be equated between these classes.

I must, unfortunately, refrain from entering into the

many relations—some already known, some new—which may by this principle be directly written down, but which formerly could be deduced only by more or less clumsy calculations. I must also forbear from discussing the new phases which have been exhibited in the light of general energetical considerations by the other earlier laws of thermodynamics—the most extended branch of Energetics. All these things must indeed be so if what I said concerning the importance of this new way of considering nature is well founded.

There is a final question which I would not like to pass untouched. When we once succeed in grasping in its pure entirety a fruitful and important truth we are only too apt to look upon it as all-comprehensive. This mistake we see daily committed in science, and the conception to the combating of which I have devoted the half of the time allowed me has arisen from exactly such a cause. We shall forthwith have to ask ourselves the question: Is energy, necessary and useful as it is for the understanding of nature, also *sufficient* for this purpose? Or are there phenomena which cannot be completely represented by the known laws of energy?

I do not think I can better justify the responsibility which I have by these remarks incurred towards you than by emphasising the fact that this question must be answered in the *negative*. Immense as are the advantages possessed by the energetical conception of the universe over the mechanical or materialistic, it seems to me, nevertheless, that already certain points may be noted which are not covered by the known laws of energy, and which therefore point to the existence of principles which extend beyond these. Energetics will remain beside these new laws; but it will not be in the future, as we must to-day consider it, the most comprehensive principle ruling natural phenomena; it will perhaps appear as a special case of a still more general relation, of the nature of which we can at present have scarcely an inkling.

I am not afraid of having lowered by what I have said the value of that mental progress which we were discuss-

ing; I think I have rather raised it. For once again are we met by the fact that Science may never recognise bounds to her progress, and that amidst the struggles for some new possession her eyes must not become blinded to the fact that beyond the territory she is striving to conquer still wider plains extend which later must also be subdued. Let us have done with the time when the smoke and dust of battle confined our vision to the narrow limits of the combat. To-day that is no longer allowed; to-day we fire with smokeless powder—or rather ought to do so—and have, therefore, with the possibility, also the duty not to fall into the errors of past periods.

WILHELM OSTWALD.

THE DIAGNOSTIC AND THERAPEUTIC USE OF MALLEÏN.

THE early investigators who raised the study of micro-organisms to the rank of a science were, for the most part, content with establishing the pathogenic or non-pathogenic characters of bacteria, and morphological considerations played an important part in the chain of evidence by which it was proved that definite species of bacteria may stand in direct causal relation to disease. At the present time, however, the growth of knowledge concerning bacteria chiefly takes place by the application of methods which are identical with those which have created physiology, and the interest which surrounds the discovery of a new micro-organism soon disappears, because researches conducted upon physiological lines are absolutely necessary before the full importance of the discovery can be appreciated. The conditions which govern its spread and mode of reproduction have to be determined, but, above all, the exchange of material which a microbe exhibits needs investigation, and it is this problem which has attracted so many workers.

The products of bacterial life have in several cases been proved to be even more harmful than the microbes themselves, while, on the other hand, filtrates of bacterial cultures have been employed to confer immunity, a state which, when established, may be transferred from one animal to another of the same or even a different species. Apart from changes in the culture media which are produced by the exchange of material consequent on bacterial life, the microbes themselves may die, and by subsequent disintegration give rise to substances which possess neither toxic nor protective properties, but are capable of causing profound constitutional changes among which fever and local inflammatory disturbances are prominent symptoms. To this class of bodies belong those bacterial proteïnes which Buchner, Römer and others have investigated, and pro-

bably tuberculin and mallein may be included in the same group. The latter is obtained from cultures of bacillus mallei, the specific micro-organism of glanders, a disease especially fatal to horses and easily communicable from them to other animals by direct inoculation. The large carnivora in menageries, lions, tigers and leopards, also succumb after the ingestion of glandered horse flesh (1), though it is still an open question whether a true infection occurs at the surface of the uninjured intestine, since the possibility of minute wounds of the mucous membrane produced by splinters of swallowed bone cannot be excluded. Babes and Cornil have, however, proved that cultures of bacillus mallei incorporated with vaseline or lanoline can be rubbed into the intact skin, conjunctiva or nasal mucous membrane and so produce glanders, and the former observer considers that infection may naturally occur by the entrance of micro-organisms into the body by way of the hair follicles.

Like other infective diseases glanders in the horse, man, or other animals may run an acute or chronic course, and an animal suffering from this malady may exhibit clinical signs which are insufficient to establish a definite diagnosis. Since the disease is common among horses, and apart from other considerations these are the animals which chiefly furnish antitoxic serum for the treatment of diphtheria and tetanus, it is a matter of the utmost importance to definitely ascertain if possible whether an individual is suffering from expressed or latent glanders. Mallein has been extensively employed both in this country and abroad to answer this question before the animals are subjected to the process for conferring immunity.

Many years before the connection between specific micro-organisms and infective diseases was clearly made out, Youatt recommended that doubtful cases of glanders should be diagnosed by reproducing the disease in the ass by inoculation with the nasal discharge of a suspected animal. The discovery of a specific microbe by Löffler and Schütz and its characteristic growth on potato firmly established the value of this method, and as an experimental

animal the guinea-pig or cat may be chosen. The behaviour of bacillus mallei in the frog is so peculiar that Sacharow (2) advises the introduction of pieces of suspected material into the abdominal cavity of this animal. In fifty-eight experiments either bouillon-cultures of bacilli or pus from fresh glander nodules were injected. In no single case did any frog become diseased, but after intervals of from two to fifty-five days the animals were killed, and without exception pure cultures of bacillus mallei could be obtained from the liver, spleen, kidneys or blood; the bile especially contained countless numbers of bacilli. This observer has not practically tested the value of his method with the nasal secretion where numerous other micro-organisms are associated with bacillus mallei, but the suggestion of Straus that the male guinea-pig should be used has frequently been followed in the diagnosis of doubtful cases. In this animal a local suppuration can be set up in two to three days after the peritoneal injection of suspected material, and specific microbes can be separated from the pus. In undoubted cases of glanders, and also in those where no ulceration or nasal discharge exists, excision of suspected nodules and application of the methods briefly indicated above may convert doubt into certainty; but the employment of malleïn as a means of diagnosis in equivocal cases promises to be of even greater value, and sufficient experimental work has now been carried out in England, France, Germany and Russia to permit some conclusions to be drawn on this subject.

About four years have now elapsed since Kalning of Dorpat (3) prepared a fluid from cultures of bacillus mallei that had been raised upon potato. The growth was removed, thoroughly mixed with water, and the mixture then heated for twenty minutes at a temperature of 120° C. for four successive days. After having been left in the incubator for two days at 39° the liquid was filtered and the filtrate sterilised by heating to 120° for fifteen minutes. Using the term introduced by Babes (4) this fluid is known as malleïn. In April, 1890, Helman had previously made a short communication on the use of

an extract of bacillus mallei as a means for conferring immunity, and he noticed that a definite rise in body temperature and local swelling followed a subcutaneous injection. In the succeeding year he more particularly indicated its value as a means of diagnosis. Both these Russian observers worked independently with the aim of rendering animals immune to glanders, and published their results before Koch made his communication on tuberculin, but this gave an enormous impulse to the study of malleïn and various solid and fluid preparations of this substance have now been obtained. Pearson (5), Preusse (6) who followed Koch's procedure for tuberculin, Johne (7), Gutzeit (8), Preisz (9), Bonome and Vivaldi (10), Malzew (11), Macfadyean (12), Roux (13), Foth (14) and Kresling (15) among others have prepared malleïn, though at the present time in France, Russia and Germany, malleïn of Roux, Preusse and Foth is chiefly used. Two of these are liquids and the last is a powder with a faint yellow tint.

Malleïn like tuberculin obviously must consist of soluble bodies derived from the microbe itself and from the products of bacterial activity as well as substances in the original culture media, and repeated attempts have been made to isolate a specific body from this complex material. Dialysis of fluid malleïn shows that both the substances which remain and those which pass through the membrane are physiologically active, and an alcoholic precipitate of malleïn acts precisely like the original material (15). A chemically pure malleïn has not yet been seen, and attempts to obtain such a body from proteid-free nutrient media have hitherto been failures (16). Malleïn undoubtedly contains nitrogenous bodies some of which belong to the proteid group, such as the bacterial proteïnes and albumoses which are possibly identical, or as Buchner points out the proteïnes contain albumoses. Both these substances are capable of exciting fever when injected into animals, and numerous bacteria besides bacillus mallei can produce them. If albumoses are separated from a filtrate, injection of these produces less febrile disturbances than the original filtrate (17). Matthes has recently attempted to decide the ques-

tion whether albumoses are only by-products or the active fever-producing principle. He obtains these bodies not by the activity of micro-organisms but by artificial gastric and pancreatic digestion, and finds that the injection of ·3-1 gramme of deutero-albumose under the skin of a healthy guinea-pig originates a fever which lasts from seven to fifteen hours (18). Other experiments conducted on the same lines lead this observer to the conclusion that an albumose is the real phlogogenic agent in such preparations as tuberculin and probably malleïn. It is necessary to be cautious in inferring this since it is an undecided question whether there is one specific substance produced by all bacteria, and, further, guinea-pigs are exceedingly sensitive to bacterial products which act differently or may cause no effect in other animals.

The action of various bacterial proteïnes and malleïn has been recently investigated by Schattenfroh (19). The proteïnes were obtained from potato-cultures of Friedländer's pneumobacillus, bacillus pyocyaneus, and the bacillus of rhinoscleroma, by methods similar to those originally employed by Buchner. These were contrasted with Foth's malleïnum siccum, and a proteïne obtained from cultures of bacillus mallei. The experiments were conducted on healthy guinea-pigs and on others which, having been inoculated with bacillus mallei, were in a state of acute glanders. He finds that the rise of temperature which succeeds an injection of malleïn occurs with all the other proteïnes, and also with a peptone which can be artificially made from fibrin or vegetable albumin. Both glandered and healthy guinea-pigs react in this way, but the febrile attack, though more intense in diseased animals, is not different in character. The specific proteïne of bacillus mallei is also found to act precisely like the proteïnes of other micro-organisms, and this is in accord with the researches of Buchner, who contends that the proteïnes of both pathogenic and non-pathogenic bacteria have an equal faculty for producing fever. It is, therefore, sufficiently obvious that Schattenfroh considers that malleïn has no essential specific action on glandered animals in consequence of the presence of a

peculiar proteïne, but that other bodies associated with this may assist to produce a reaction which is considered as diagnostic.

These experimental results were obtained by working with one species of animal, and, according to Schindelka (20), they are not true for horses. On injecting proteïnes identical with those employed by Schattenfroh into three horses free from glanders, a considerable febrile attack was produced, and on a glandered horse the effect was negative. Malleïn acted in an inverse manner, and this observer considers that inquiries into the relative value of malleïn and other bacterial products only strengthen the evidence in favour of malleïn.

The interest which attaches to bacteriological research lies in part, if not wholly, in the solution of the problem of immunity, and in the experimental work which directly aids in the treatment of disease. In glanders neither of these aims has as yet been accomplished. The results obtained by Babes, who claims to have rendered guinea-pigs immune to glanders by treatment with minute injections of malleïn, have not been confirmed. According to the same observer guinea-pigs can also be cured of glanders by administration of this substance; and two horses suffering with the chronic form of the disease were also cured. The majority of observers are, however, agreed that no therapeutic effect follows treatment with malleïn. Favourable results may follow, but since spontaneous recovery from this disease may take place a possible fallacy is not excluded. Schattenfroh and others have shown that malleïn has no therapeutic power, and in this as well as in many other characteristics this substance resembles tuberculin.

As a diagnostic measure malleïn appears to have an assured position. A recent paper, published by Foth (21) last August, reviews this question at length. Nearly all observers point out that animals suffering with acute or chronic glanders, or even with the mild condition spoken of as latent, may exhibit a reaction to small doses of malleïn. The symptoms produced in glandered horses, according to

Nocard, may be grouped into organic and thermic, and these may last twenty-four to thirty-six or even forty-eight hours (22) after a subcutaneous injection. A variable amount of stupor and prostration follows, and at the site of injection an inflamed swelling which shows no tendency to suppurate may form. Assuming that the animals have no initial fever, Nocard considers that the thermic effects following injection may be classified in three groups according to the intensity of the febrile disturbance, which may vary from ·2° to 2° C. above the normal, this being 37·5° to 38° C. for the horse. Schindelka of Vienna (23), whose experiments were made with Foth's malleïn, especially points out that a "typical" temperature curve is yielded by glandered animals on injection of ·1 gramme of malleïn. If the temperature reaches or exceeds 2° C. above the normal, and if from this level the temperature slowly falls, the animal is certainly glandered. Even if a healthy animal or one suffering with pneumonia or other pulmonary diseases reacts with a fever of 2° C., the normal temperature is reached with an abrupt and not a gradual fall. Foth has given a *resumé* of the experiences of malleïn injection. Four preparations of this substance were employed: a malleïn derived from cultures of bacillus mallei on potato (Preusse of Dantzic and Preisz of Buda-Pest), from bouillon cultures (Roux of Paris, Kresling of St. Petersburg), and the malleïnum siccum of Foth. Macfadyean in this country has also employed malleïn of Roux and a preparation obtained by himself from bouillon cultures. The results may be arranged in the following manner, and these appear to show that even if an injection of malleïn is not an infallible means of diagnosis, it is certainly one of unrivalled value when contrasted with the ordinary clinical and bacteriological methods of investigation.

A. Experiments with Preusse's or Preisz's malleïn.

Total number of horses injected . . .	457	
Reaction to malleïn 1° C. and over	303	259 glandered. 44 not glandered.
Reaction slight or negative . .	154	132 not glandered. 22 glandered.

B. Experiments with Kresling's malleïn.

Total number of horses diagnosed with malleïn 658

Marked reaction in 230, of these 14 killed, all glandered.
Slight ,, 138 ,, 7 ,, ,, ,,
Negative ,, 290 ,, 1 killed, healthy.

C. Experiments with the malleïn of Roux. These have been chiefly carried out by Nocard of Alfort at the horse depôt at Montoire, and at the stables of the Compagnie l'Urbaine in Paris.

Experiments at Montoire.

Total number of horses injected with malleïn, 233. Each animal was injected three times at intervals of a month.

Group i. Reaction Temp. 1·9°–2° C.
,, ii. ,, ,, 1°–1·9° C.
,, iii. ,, ,, less than 1° C.

Ninety-two horses from all these groups were killed and examined. All from group i. were glandered and eleven others from groups ii. and iii.

Experiments in stables of Compagnie l'Urbaine.

Total number of horses subjected to malleïn 4348
Horses proved to be glandered . . 523
Reaction temperature of these :—

3° C. and over 45
2·5°–2·9° C. 96
2°–2·4° C. 240
1·5°–1° C. . . , . . 142

D. Schindelka, Rudovsky and others have used the malleïnum siccum of Foth.

Horses injected 669
Horses examined post mortem . 252
Horses which reacted by fever of 2° C. and over, 96 per cent.
Horses which reacted negatively, 4 per cent.
All horses which reacted by 2° C. and over were proved on post mortem to be glandered.

All horses which reacted by less than 1·3° C. were proved to be free from glanders.

All horses reacting by temperatures 1·3°–1·9° C. were on autopsy found to be either glandered or not in almost equal proportion.

E. The experiments of Macfadyean and Hunting (24) were made with Roux's malleïn on thirty-nine horses. At the time of their experiments London had the unenviable notoriety of possessing more glandered horses than any other city in Europe. Subsequently the former observer continued his experiments on fifty more animals, using malleïn prepared by himself, by Foth and by Roux (25). He considers that malleïn is of incomparable diagnostic value, and even if it is not an absolute method, this offers the only possible solution for obscure cases. If the temperature of a fever-free animal reaches 40° C. and local inflammation occurs at the site of injection the animal is almost certainly glandered. A rise of 1° C. with local symptoms furnishes evidence of probable disease, but if injections of malleïn are carried out on horses that have fever the indications to be drawn from the experiments are uncertain.

All work that is still in the experimental stage can only be strengthened by continued research, and that undertaken by Schütz (26) and Prús does not accord with the favourable results obtained by Semmer, Johne, Heyne and others. The former observer used the malleïn of Preusse in doses of ·5 c.c. on fifty-eight horses: all were killed and twenty-two examined post mortem. Fifteen of these had given typical malleïn reaction, that is to say a rise of temperature up to 1·5° C. and higher with a succeeding gradual fall. Seven horses had reacted with a rise of 1°–1·4° C. No single animal was glandered. This stands in striking contrast to the results of the majority of observers, although Prús (27) states that malleïn has no specific influence in glandered animals and possesses no diagnostic power. The discrepancies which exist on this question even if fully recognised leave no doubt that malleïn has a very definite value in diagnosis. The conflicting statements of Schütz

may well depend on the quality of the mallein used. Even if this is not the case the experiments only show that non-glandered animals may respond with rise in temperature, and it has repeatedly been noticed that animals suffering with acute respiratory troubles may give a reaction which resembles the specific effect of mallein.

In veterinary practice tuberculin is freely used as a diagnostic measure for cases of bovine tuberculosis, and Arloing (28) has studied the effect of injections of pneumo-bacillin in suspected pleuro-pneumonia. Neither of these bacterial products appears to possess any therapeutic value, and the same is certainly true for all the various preparations of mallein. The employment of this substance for therapeutic purposes appears useless, but these researches have at the same time shown its value as a means of diagnosis in glanders. A considerable literature deals with this question, and Commissions (29) have reported in favour of its use. Even if it is admitted that the experimental work is not absolutely convincing it may be fairly urged that in the majority of cases where the clinical symptoms are obscure, injections of mallein offer the surest test by which doubtful cases can be recognised.

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GEORGE A. BUCKMASTER.

THE SPACE RELATIONS OF ATOMS.

(*Concluded.*)

THE coincidence of optical activity with molecular dissymmetry having been established, as regards, at least, the great majority of compounds of the type $CR'R''R'''R^{iv}$, the question arose: Can the degree of asymmetry be measured by the degree of optical activity?

But in view of the fact that the change of rotation resulting from a change of conditions (temperature, wave-length of light, solvent, concentration) is not the same for different substances, the preliminary question arose: Under what conditions can comparable numbers be obtained for the optical activity of various substances?

The most important variations are those produced by the nature and proportion of the solvent; in the attempt to avoid these variations by examining the pure substance, a greater difficulty arises from the action which the molecules of the substance itself are then free to exert on one another. This compels a resort to dilute solutions, and the difficulties here encountered are greatly diminished if we consider the dissolved substances as divided into two classes, electrolytes and non-electrolytes—a division forced upon us by the application of the theory of electrolytic dissociation.

As regards electrolytes the theory teaches that different salts of the same active acid or base possess in dilute solution the same molecular rotation, because they contain approximately the same number of active ions; this identity of rotation is the law experimentally established by Oudemanns and Landolt.

For non-electrolytes the results obtained lack precision, because the molecular weight of the active body, and the possible action of the solvent, have in many cases still to be determined.

The effect of temperature and of wave-length is of minor importance, and, by using the same solvent in the same proportion under similar conditions, comparable results may be

obtained, at least for bodies of similar constitution. And the attempt to apply these results to the determination of the degree of molecular dissymmetry has been made by Guye.

Assuming for the body CR_4 the form of a regular tetrahedron, it will have six planes of symmetry, at the intersection of which will be the centre of gravity of the molecule. In a body $CR'R''R'''R^{iv}$ the C.G. of the molecule is removed from all the six planes by distances d^1, d^2, d^3, d^4, d^5, d^6. The product of these distances is the measure of the dissymmetry (*i.e.*, of the activity of the molecule).

This theory fulfils the condition that when two groups become identical the activity must vanish, for in this case one of the distances, d^1, d^2, etc., becomes *nil*, and their product is likewise *nil*.

The distances d will be determined in the first place by the relative weights and distances of the groups R, and as it is the relative weights alone which can at present be determined, the first step was to see how far the distances d depend on the difference of the weights R: in short, whether for the equation

$$P = d^1 \times d^2 \times d^3 \times d^4 \times d^5 \times d^6$$

we may substitute

$$P=(R'-R'')(R'-R''')(R'-R^{iv})(R''-R''')(R''-R^{iv})(R'''-R^{iv}),$$

where P is the "asymmetry-product" or the measure of the dissymmetry of the tetrahedron.

Assuming this formula we get the following results :—

(1) If two of the groups R change places the activity remains the same, but the sign changes, as the existence of isomers of equal and opposite activity demands.

(2) If the group-weights change, but their order remains the same, the magnitude of the rotation will change but not the sign.

(3) If the heaviest group, say R^{iv}, be replaced by groups of constantly decreasing weight the rotation (say positive) will decrease, and will reach *nil* when R^{iv} becomes equal to the next heaviest group R'''. Beyond this point the rotation (now negative) will increase, declining again to *nil* when $R^{iv} = R''$, and changing sign again when this point is passed,

and so on ; so that as R^{iv} gradually passes from the maximum to the minimum the sign of the rotation changes four times.

These conclusions have been tested in the case of over forty derivatives of amyl alcohol. If in amyl cyanide,

$$(29)C_2H_5—\underset{\displaystyle H}{\overset{\displaystyle CH_3(15)}{\overset{|}{\underset{|}{C}}}}—CH_2CN(40),$$

the largest group, CH_2CN, be replaced by others of weight exceeding 29, the molecule remains in almost every case dextro-rotatory, and in most cases the rotation increases with the mass, as shown by the following examples :—

	amyl cyanide.	amyl chloride.	caproïc acid.	amyl bromide.	amyl iodide.
CH_3 \ $C_2H_5—C-$ / H	$-CH_2CN$	$-CH_2Cl$	$-CH_2COOH$	$-CH_2Br$	$-CH_2I$
	40	49·5	59	94	141
α_D	$+1^\circ 16'$	$+1^\circ 6'$	$+3^\circ 20'$	$+4^\circ 24'$	$+8^\circ 20'$

But on the other hand we have

amyl alcohol $-CH_2OH\,(31) = -5^\circ 2'$
,, amine $-CH_2NH_2(30) = -3^\circ 30'$
,, aldehyde $-COH\quad(29) = +0^\circ 42'$.

So that the change of sign which should take place only below 29 begins at 31.

Thus, as Guye points out, the molecular dissymmetry depends largely, but by no means exclusively, on the weight of the groups. The study of other compounds (esters of tartaric, glyceric, and valerianic acids) leads to the same conclusion. In particular it is noticed that equality in the weight of two different groups is not, as the formula demands, accompanied by inactivity. Thus ethyl-malic acid,

$$\begin{matrix} C_2H_5O \\ COOH \end{matrix}\!\!\diagdown\!\!\diagup C \!\!\diagup\!\!\diagdown \begin{matrix} CH_2COOH \\ H \end{matrix}$$

exhibits dextro-rotation, although $C_2H_5O(45) = COOH(45)$. Similarly amyl aldehyde remains slightly active, although $COH(29) = C_2H_5(29)$.

Again, propyl-glycerate is more active than iso-propyl-glycerate. It might be supposed that this was due to the

moment of the chain $CH_3CH_2CH_2$— about the central carbon atom being greater than that of the chain $\begin{matrix}CH_3\\CH_3\end{matrix}\!\!>\!\!CH$; unfortunately for this explanation it is found that iso-propyl-tartaric acid is more active than propyl-tartaric acid; and Freundler (*Bull. Soc. Chem.* [3], ii., 305 and 366) finds that in general the calculation of the moments of the carbon chains gives no satisfactory results.

It remains to mention the theory of Crum Brown, who proposes to determine, for each of the radicals linked with carbon, a function K, the "rotation-equivalent," a function which perhaps varies with the temperature, etc.; the difference between these functions would determine the rotation of the molecule. The rotation-equivalent is considered by Crum Brown as a function of the composition and constitution of the radical, not necessarily proportional to the mass and the distance of the centre of gravity from the centre of the carbon atom. This hypothesis, as its author observes, takes no account of the mutual action of the radicals, and it is unlikely that a radical will in every compound have the same rotation-equivalent, its distance from the central carb n atom not being always the same. In closely allied compounds, however, it seemed possible that the effect of such differences would be inappreciable, that in them the rotation-equivalent of each radical might be constant. Acting on this supposition, J. Wallace Walker[1] has recently found that in the ethereal lactates there is a constant increase of 5·5° in the rotation of the molecule for each addition of CH_2 to the carboxyl group. In the brom-propionates the same addition causes a much greater increase of rotation. Thus ethyl brom-propionate has a rotatory power 14·3° greater than the methyl compound, and propyl brom-propionate 14·2° greater than the ethyl compound. Further, almost all bromine derivatives are found to have a greater rotation than the corresponding chlorine derivatives, showing that the rotation-equivalent of bromine is greater than that of chlorine.

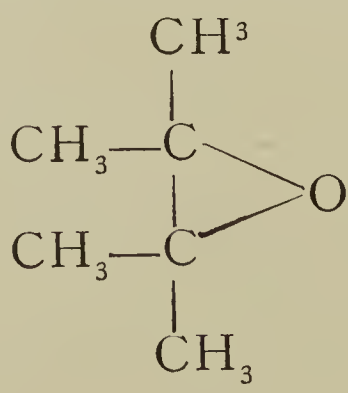
So far our review of stereochemistry has occupied itself

[1] *Journ. Chem. Soc.*, p. 914, 1895.

mainly with stereo-isomerism; it has concerned itself only with such bodies as could be isolated in numbers unaccounted for by any possible application of structural formulæ. In many cases the configuration attributed to these bodies by the tetrahedron hypothesis has been confirmed by the study of their reactions.

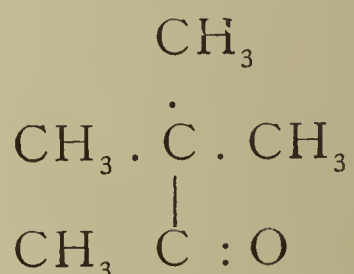
But we have now to consider cases in which the study of reactions has been the first step, and has led to stereochemical theories, such as have been developed more especially by Bischoff under the title of "the dynamic hypothesis". This name, chosen because the object of these studies was to determine the motion of the atoms, as well as their average position, seems appropriate also because the subject of the investigations has been the reactions of bodies, rather than the number of their isomers—chemical dynamics rather than statics.

The phenomenon of ring-formation has already afforded a striking example of the connection between reactions and the space relations of the atoms concerned. The formation of a carbon ring has been seen to depend on the number of its members, and on the relative position (cis- or trans-) of the groups whose interaction closes the ring. But, as Bischoff points out, the nature of the atoms connected with members of the ring not directly concerned in the closing thereof has also an effect. Thus a methyl group substituted for hydrogen in the system of succinic, of maleïc, or of glutaric acid facilitates the closing of the ring,—formation of an anhydride. In other cases, however, methyl prevents or renders difficult the ring-formation. Sometimes, indeed, in a compound containing several methyl groups it is easier to obtain a molecular re-arrangement than a simple ring-formation. Thus instead of



I.

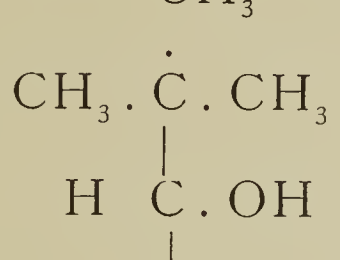
we may obtain



II.

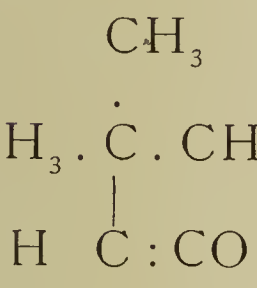
For such a case static formulæ, whether structural or tetrahedral, have no ready explanation. Bischoff's explanation is that in formula II. the methyl groups can oscillate much more freely than in I. It is II. therefore which must be formed according to the "dynamic hypothesis" which states that "those configurations are the most favoured in which the components of the system can oscillate most freely".[1]

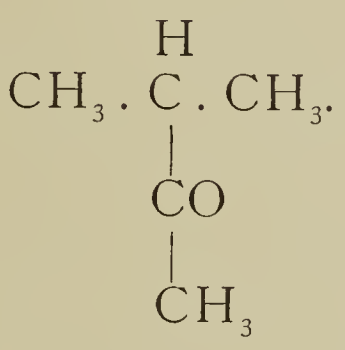
In the cases mentioned above, where the methyl groups cause closure of the ring, it is again their effort to gain room for their oscillations which causes them to crowd together the hydroxyl groups, so that expulsion of water with ring-formation follows. The dispersion of the methyl groups is observed also in open chain compounds. Thus β-tri-methyl ethylidene lactic acid



$COOH$ yields

not $CH_3 . C(CH_3) . CH_3$, but





Bromine atoms also tend to separate as widely as possible. Thus α-di-brompropionic acid becomes α-β-brompropionic acid

$$CH_3 . CBr_2—COOH = Br . CH_2—CH(Br)—COOH.$$

[1] It may be added that as like atoms have like oscillations, they will be the most prone to collide, and in a favoured configuration they must be far removed from one another. Moreover, atoms which though unlike are of like nature (degree of positivity) must to a less degree be far removed from one another.

In the second arrangement the oscillations of the bromine atoms are evidently less subject to mutual hindrance.

As in intra-molecular so in inter-molecular transformations the same phenomenon is observed. As one example among many it may be noted that brom-isobutyric ether re-acts with silver in two ways, thus—

$$C_2H_5CO_2 . \overset{\overset{CH_3}{\cdot}}{\underset{\underset{CH_3}{\cdot}}{C}}—Br + Ag_2 + Br—\overset{\overset{CH_3}{\cdot}}{\underset{\underset{CH_3}{\cdot}}{C}}—CO_2C_2H_5 = Ag_2\ Br_2, \text{ and}$$

$$\text{either I. } C_2H_5CO_2 . \overset{\overset{CH_3}{\cdot}}{\underset{\underset{CH_3}{\cdot}}{C}}—\overset{\overset{CH_3}{\cdot}}{\underset{\underset{CH_3}{\cdot}}{C}} . CO_2C_2H_5,$$

$$\text{or II. } C_2H_5\ CO_2 . \overset{\overset{CH_3}{\cdot}}{\underset{\underset{CH_3}{\cdot}}{C}}—CH_2—\overset{\overset{CH_3}{\cdot}}{\underset{\underset{H}{\cdot}}{C}} . CO_2\ C_2H_5$$

According to Bischoff the dynamic hypothesis signalises II. as the favoured configuration, and demands that it be formed in larger quantity; but an answer to the question why the unfavoured configuration should be formed at all is still lacking. We may hope, however, with Bischoff, that in time stereochemistry will enable us to foretell the course of "abnormal" reactions just as structural chemistry enables us to foretell the course of ordinary re-actions, and we shall then be able to improve the models now in use which are deficient as regards the representation of the distances of the atoms from one another.

In connection with the subject of atomic motion there is to be mentioned in the first place the simple and wide view of Le Bel, which supposes connected atoms or their repulsive zones to glide freely over the surface of one another in every direction, no directive force acting from one to the other. This is not inconsistent with the limitations which the atoms of a molecule may impose on one another in the

way indicated by Bischoff. In one case at least, however, there is a limitation of motion which seems due to a directive force, and which tells in favour of the tetrahedron hypothesis. The existence of the stereomeric inactive trioxyglutaric acids corresponding to the types

$$\begin{array}{cc} R'' & R'' \\ R'CR''' & R'CR'' \\ XCY & \text{and } YCX \\ R'CR'' & R'CR''' \\ R'' & R'' \end{array}$$

proves that the groups attached to the carbon atoms do not rotate about one common axis; *i.e.*, as the tetrahedron hypothesis demands, the lines joining the carbon centres are not in a straight line.

Before leaving the stereochemistry of carbon it is worth while to notice the calculations of O. E. Meyer in his *Kinetische Theorie der Gase*,[1] concerning the arrangement of the atoms in various molecules. According to his results molecules may be divided into four classes: (1) the volume of the molecule is equal to the sum of the molecules of the atoms; (2) the area of the molecule is equal to the sum of the areas of the atoms; (3) the diameter of the molecule is equal to the sum of the diameters of the atoms; (4) for some molecules none of these relations hold. In molecules of the first class we must suppose that the atoms are arranged symmetrically in every direction, so that they occupy as nearly as possible a sphere. Such a molecule according to Meyer is CH_4, which may be represented with the four hydrogens symmetrically arranged on the surface of a sphere, and rotating about the carbon atom at the centre—a confirmation of the tetrahedral hypothesis. In molecules of the second class the atoms must be and move in one plane, and the figures obtained for NH_3 show this to be such a molecule—another confirmation of current theory. An example of the third class is H_2O, which we must therefore suppose to have the atoms arranged in a straight line. HCl satisfies all three of the previous conditions almost equally; this indicates that the two atoms lie so close together that

[1] Breslau, 1877, Maruschke and Berendt.

the short line joining them is almost a point. The fourth class is composed of more complicated molecules, as CH_3Cl, C_2H_4, C_2H_6O, C_2H_5Cl, to which, therefore, the form of flattened sphenoids is attributed.

. . . . . .

The stereochemistry of elements other than carbon has made little progress, except in the case of nitrogen. Speculations on the space relations of nitrogen compounds have long been rife, but it is only of recent years that they have acquired practical interest. In the case of trivalent nitrogen, as in the case of carbon, it was the discovery of isomers unforeseen by the structural formulæ which led to the new doctrines. These mysterious nitrogen isomers were found particularly among the oximes, $\begin{matrix}R'\\R''\end{matrix}\!\!\rangle C = NOH$, and here it is constantly observed that when R′ and R″ are not identical, an isomerism arises which is lacking when identity is established. Thus aldoximes exhibit this isomerism, but it is lacking in $\begin{matrix}H\\H\end{matrix}\!\!\rangle CNOH$; again, it is noticed in nineteen non-symmetrical ketoximes but is lacking in the di-phenyl compound. When the number of $=CNOH$ groups in the molecule increases, the number of isomers also increases, as in the case of benzil dioxime and phenyl glyoxime. (Sometimes, however, only two isomers can be isolated.) Later, the phenyl hydrazones, $\begin{matrix}X\\Y\end{matrix}\!\!\rangle CNNHC_6H_5$ and $\begin{matrix}X\\Y\end{matrix}\!\!\rangle CNN(C_6H_5)_2$, were found to present similar isomerism, provided always that the groups X, Y, were dissimilar. Observing that this dissimilarity was essential to the occurrence of the isomerism, Hantzsch and Werner were led to explain it by the formulæ

$$\begin{matrix}RN\\\|\\XCY\end{matrix} \qquad \begin{matrix}NR\\\|\\XCY\end{matrix}$$

In one isomer the radical R attached to the nitrogen atom is attracted towards X, in the other towards Y, the nitrogen valences being diverted by these attractions from the plane which, according to reasoning previously given, they ordinarily occupy; and there is always a difference

between the two isomers which may be supposed to correspond to the difference in the attractions X—R and Y—R.

A stereochemical formula for the carbon nitrogen compounds in question is obtained by Van't Hoff by joining the nitrogen triangle to the carbon tetrahedron as shown in figure 12.

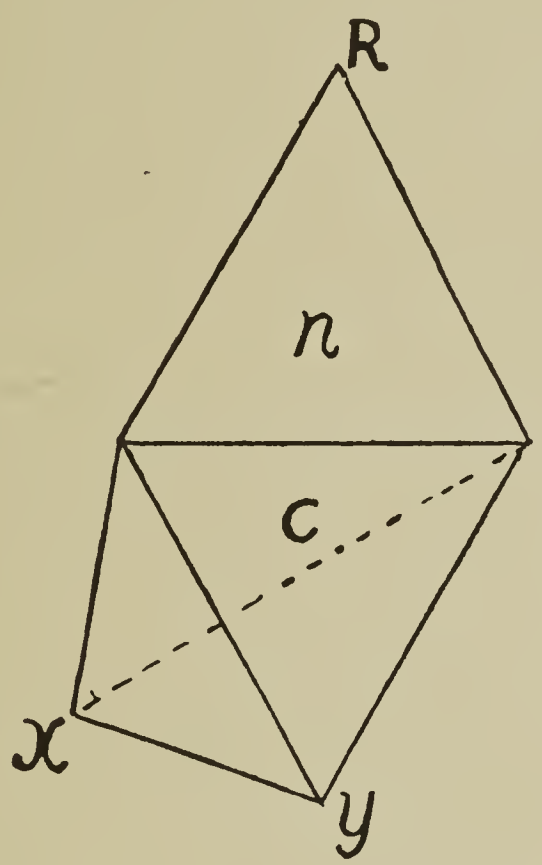


FIG. 12.

Here R is represented in the position it would have if X and Y were identical; when they differ, one or the other isomer results according as R approaches X or Y. There is no evidence to show whether N is or is not attracted to the same side as R.

For the three benzil dioximes Hantzsch and Werner give the formulæ

XC—CX	XC—CX	XC — CX
‖ ‖	‖ ‖	‖ ‖
RN NR	RN RN	NR RN
Anti-dioxime.	Amphi-dioxime.	Syn-dioxime.

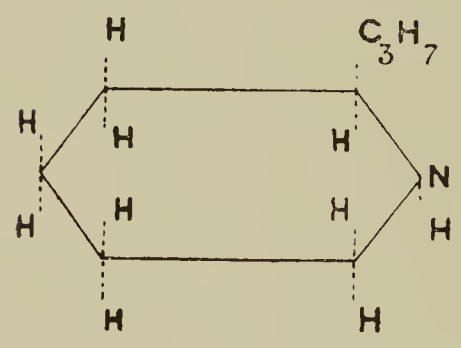
In most cases these chemists have been able by a study of the reactions of the isomers to determine which of the formulæ belongs to each. For instance, of the two formulæ

$$\begin{matrix} R—C—H \\ \| \\ N.OH \end{matrix}$$ (syn-aldoxime) and $$\begin{matrix} RCH \\ \| \\ HON \end{matrix}$$ (anti-aldoxime), the

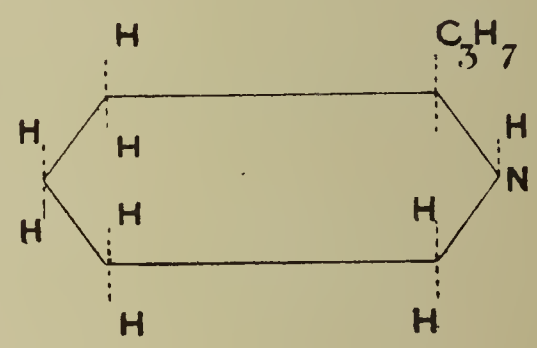
first must be assigned to the isomer which alone yields the nitrile, according to the equation

$$\begin{array}{ccccc} RCH & & RC & & H \\ \| & = & \| & + & \| \\ NOH & & N & & OH \end{array}$$

In ring-compounds a similar isomerism, corresponding to the cis-trans isomerism of the hydrophthalic acids, has been observed. Thus Ladenburg attributes to coniine and iso-coniine the formulæ



and



The isomerism of the kreatinines is probably analogous; here as well as with the coniines one of the chief distinctions between the isomers is the difference in solubility of the platinum double salts.

With regard to pentad nitrogen it has already been observed that, united to five different radicals, it causes optical activity. When any two of the radicals are identical the activity disappears; but now another kind of stereomerism may arise. Thus two trimethyl isobutyl ammonium chlorides and two methyl diethyl amyl ammonium chlorides have been formed. When the radicals are smaller this isomerism does not occur; thus there is only one trimethyl propyl and one tripropyl methyl ammonium chloride, probably owing to the mobility of the smaller radicals.

The existence of the active isomers shows that in them the atoms are not in one plane, and the existence of the other isomers shows that an alkyl may occupy either of two positions in a substituted ammonia. The inactivity when two radicals are identical shows that these lie symmetrically with regard to the rest of the molecule. A graphic repre-

sentation of these relations is given by Van't Hoff in the figure.

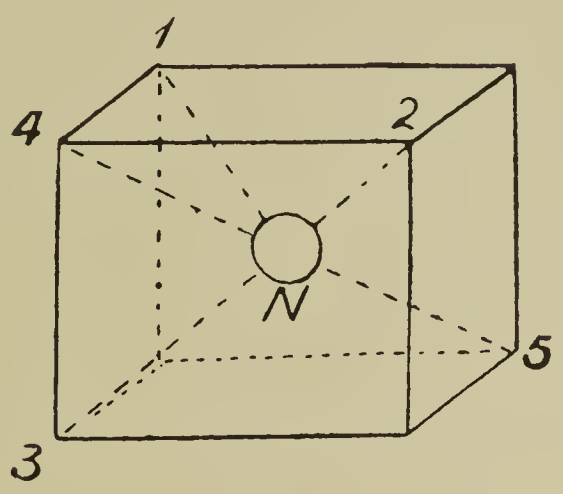


FIG. 13.

Here the nitrogen occupies the centre of the cube, and the five radicals connected with it occupy five of the corners. Of these, 1, 2, and 3, which have like positions, represent alkyls held by the three ordinary valences. When the nitrogen is triad these lie in one plane with the nitrogen atom; in the figure they are shown nearer to 4, as they would be attracted by the negative atom, say chlorine, occupying that position. The fourth positive group (alkyl) would occupy 5.

Now for the compound $NR'R''R'''R^{iv}Cl$ a symmetrical arrangement is impossible. Four types are possible, as any one of the four radicals may occupy 5, and each type should be divisible into two isomers of opposite activity. Already several isomers have been prepared in the case of $N . C_4H_9 . C_3H_7 . C_2H_5 . CH_3 . Cl$.

For $N(R')_2 R'' R'''Cl$ we may have either an asymmetric configuration (R′ in 5) or a symmetric configuration (R″ or R‴ in 5), and the symmetrical being always the more favoured compound it is the latter only which exists; for Le Bel found it impossible to divide these compounds into active components.

For $NR_3R''Cl$ two isomers are possible according as R″ occupies 5 or 1 (1 = 2 = 3). This is the isomerism just mentioned as occurring in the case of $N(CH_3)_3 . C_4H_9 . Cl$.

. . . . . .

With regard to the literature of this subject, very full

references are given in Bischoff's *Handbuch*,[1] and in the work of Van't Hoff so often cited. In English there is an earlier edition of Van't Hoff translated by Marsh; and in the brief *Guide* to the subject by the present writer there is also an index to the literature.

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Correction. On p. 502 the formation of active glyceric acid by means of *bacillus ethaceticus* was, in accordance with the reference given by Van't Hoff (*Atome im Raume*, p. 23), attributed to Tutton, instead of to Frankland and Trew (*Journ. Chem. Soc.*, lix., 96). The acid formed is right-handed; the salts, of which Tutton (*ibid.*, p. 233) examined one, being left-handed.

ARNOLD EILOART.

[1] Frankfurt-a-M., 1893-94. H. Bechhold.

THE SUPRARENAL CAPSULES.

THE function of a gland which has a duct is a comparatively simple physiological problem, but the use of ductless glands has long been a puzzle to investigators. Recent research has, however, shown that most of, if not all, the ductless glands do form a secretion, and this "internal secretion," as it is termed, leaves the gland by the venous blood or lymph, and thus is distributed and ministers to the needs of parts of the body elsewhere. Many of the glands which possess ducts and form an external secretion, form an internal secretion as well.[1] Among these the liver, pancreas, and kidney must be mentioned. Among the ductless glands the thyroid has attracted considerable attention, because here important practical results in the treatment of the disease called myxœdema have followed close on the heels of experimental investigation. More recently, however, the suprarenal capsule has excited interest; whether here also extracts of the gland will prove beneficial in cases of Addison's disease remains to be seen. There is already a small amount of clinical evidence in that direction,[2] but it is more in relation to the physiological aspects of the question that I desire, in this paper, to dwell.

In Dr. Rolleston's Goulstonian lectures on the suprarenal capsules delivered this year[3] there are as many as 187 references to original work on the subject; the literature is, therefore, far from scanty, but the main facts in the history of our physiological knowledge may be put in a comparatively short space. This I propose to do very briefly, and then proceed to give an account of the most recent researches in connection with the matter.

[1] The reader is referred for a succinct account of the present state of our knowledge on the general subject of internal secretions to Professor Schäfer's address to the British Medical Association, printed in the *British Medical Journal*, 10th August, 1895.

[2] See Dr. George Oliver, *British Medical Journal*, vol. ii., p. 653, 1895.

[3] *British Medical Journal*, vol. i., pp. 629, 687, 745, 1895.

The immense importance of the suprarenal bodies in nutrition was first indicated by Addison, who in 1855 pointed out that the symptoms of the disease now known by his name are associated with pathological alterations of these glands. This was tested experimentally by Brown-Séquard, who found a few years later[1] that removal of the suprarenals was rapidly and unfailingly fatal in all animals. The symptoms were practically the same (although much more acute) as those of Addison's disease, *viz.*, great muscular weakness, loss of vascular tone, and nervous prostration. These results attracted much attention, and numerous investigators set to work to verify them. Some failed to confirm Brown-Séquard's results, probably because the removal was incomplete, or accessory capsules existed. The controversy, however, soon ceased, and the subject was almost forgotten. The interest in the matter has now been revived, and Brown-Séquard's experiments have been repeated within the last few years by Brown-Séquard himself, Abelous and Langlois, Tizzoni, Schäfer, and many others. All these observations have tended to confirm the original statement of Brown-Séquard.[2] Abelous and Langlois found that the blood of animals dying in consequence of the operation is toxic for other animals which have been recently deprived of their capsules, though it causes no toxic results in normal animals. The symptoms caused by this blood are stated to be those of curare poisoning, that is, paralysis of the intramuscular nerve endings; they conclude that after removal of these glands a certain toxic product of muscular metabolism accumulates in the blood, and that the function of the glands is to remove or destroy this principle.

This is the "auto-intoxication" theory of the suprarenal capsules, and is similar to that which has been applied to the thyroid and other glands. It is chiefly founded on the fact that the blood of animals which are moribund in con-

[1] *Journ. de la physiol.*, t. i., 1858.

[2] Some few observers (*e.g.*, Pal, *Wiener klin. Woch.*, p. 899, 1894) have failed to produce death. Non-success was here, perhaps, due to the sources of failure already indicated.

sequence of the particular extirpation is poisonous, especially for other animals which have been submitted to the operation. It seems probable that the blood of an animal dying slowly from any disease would be toxic, and especially so to those animals whose resisting power has been lessened by a severe operation. Whether, however, the capsules do or do not perform an excretory function, they unquestionably produce a material which has entirely different properties from those stated to be possessed by the blood of animals deprived of these organs. This aspect of the question has been studied by making extracts of the healthy gland, and injecting these into the circulation and noting the results.

Pellacani,[1] both alone and in conjunction with Foa,[2] appears to have been the first to study the subject in this manner. The symptoms noted were chiefly respiratory, and death preceded by paralysis was in most cases the ultimate result. Marino-Zucco[3] attributed this result to neurine, which is contained in considerable amount in the capsules, and is stated to appear in the urine after the injection. None of these observers, however, availed themselves of the more exact methods of modern physiology, and give no graphic records illustrating their statements; blood pressure they seem to have neglected altogether.

This gap has now been filled up by Schäfer and Oliver[4] in this country, and later by Szymonowicz[5] and N. Cybulski[6] in Poland.

Schäfer and Oliver confirm previous workers, in so far as they show that the effects are chiefly seen in the muscular system, but the toxic effects on the voluntary muscles are those of veratrine rather than of curare. The effect of

[1] *Arch. per le sci. med.*, iii., 1874.

[2] *Ibid.*, vii., 1884.

[3] *Chem. Centralbl.*, 1888. Marino-Zucco and Dutto, *Moleschott's Unters.*, xiv. Marino-Zucco and Guaruceri, *Chem. Centralbl*, 1888, and *Arch. ital. de Biol.*, x.

[4] "Proc. physiol. Soc." contained in *Journ. of Physiol.*, xvi., p. 1, and xvii., p. ix., and *Journ. of Physiol.*, xviii., p. 230.

[5] *Anz. der Akad. d. Wiss. in Krakan*, February, 1895.

[6] *Ibid.*, March, 1895; and *Gaze ta lekarska*, 1895, No. 12, Warschan.

injecting an extract of suprarenal capsules on involuntary muscle is equally marked. The enormous rise of blood pressure which immediately follows the injection is partly due to increase of the heart's force and frequency. This is best seen when the vagi are cut; if these nerves are intact, the heart is slowed; the increase of arterial pressure is, however, chiefly due to contraction of the arterioles, and this appears to be a direct action of the poison rather than an indirect one through the vaso-motor centre. These facts are supported by very beautiful tracings taken from the heart, from the arteries by the mercurial manometer, and from peripheral organs by the use of plethysmographic instruments. Many of these are reproduced in the paper last cited.

Szymonowicz and Cybulski confirm these facts in the main. The chief point of difference is that they consider the extract acts upon the vaso-motor centre, and not directly on the blood-vessels. In confirmation of this they state that the action is not obtained after section of the spinal cord. Schäfer and Oliver find on the other hand that section of the cord makes practically no difference. The most important new facts added by Cybulski are contained in the statements that the blood of the suprarenal vein contains the active principle of the gland in sufficient amount to produce physiological symptoms, and that the changes in the circulatory and respiratory systems accompanying deprivation of oxygen are no longer obtained in animals which have been deprived a few hours previously of their suprarenals; they also find that while arterial blood does not destroy the active principle, permanganate of potash does.[1] On this fact Cybulski founds a theory that it is got rid of in the body by oxidation. Professor Schäfer has carefully tested the statements in reference to asphyxia in three cases and been unable to arrive at the same results; the absence of asphyxial symptoms observed by Cybulski was probably the result of severe shock.

[1] Moore working in Schäfer's laboratory has found that oxidation of an active extract with hydrogen peroxide materially weakens its physiological effects.

The nature of the poisonous agent is still uncertain, but the interesting facts have been further made out that it is confined to the medulla of the glands, that it is absent from the suprarenals in cases of Addison's disease, and that the physiological effects are produced by doses compared to which those of homœopaths are gigantic. Schäfer estimates that one-millionth of a gramme per kilogramme of body weight is sufficient to produce physiological effects. The effect is transitory, the poison apparently being packed away and rendered innocuous in certain organs which cannot at present be satisfactorily specified.

It should be added that the extracts of the glands used were prepared with water, with alcohol of various strengths, and with glycerine. They are made either from the fresh or dried gland, or by boiling for a few minutes. The principle is not soluble in absolute alcohol or ether. The animals experimented on were chiefly dogs, but cats, rabbits, guinea-pigs, and a monkey gave confirmatory results.

As a general conclusion, Professor Schäfer inclines to the theory of internal secretion as against that of auto-intoxication; the gland forms something which is distributed to the muscles, and is essential for their normal tone; when the gland is removed the toxic effects seen are the result of the absence of this internal secretion.

Although, as just stated, the chemical nature of this remarkable substance is still uncertain, there have been some researches directed to determining what it is. B. Moore[1] considers that it is identical with a powerful reducing substance found only in the medulla of the gland, and first described by Vulpian.[2] At any rate the solubilities of this reducing material are identical with those of the active physiological principle. It gives a dark green or blue colour with ferric chloride passing through purple to a dark red on the addition of ammonia or sodium carbonate. With chlorine, bromine, or iodine water, peroxide of hydrogen or alkalis in the presence of oxygen, it gives a rose-red

[1] "Proceedings of Physiological Society," p. xiv., 1895 (contained in vol. xvii., *Journal of Physiology*).

[2] *Compt. Rend.*, xliii. and xlv.

coloration discharged by sulphide of hydrogen or of ammonium. It is insoluble in organic solvents like alcohol, ether, or benzene; it is soluble in water, alcohol *plus* water, and dilute acids. It is found only in the medulla of the suprarenal capsule, or after death in the blood of the suprarenal vein. Its presence in physiologically active extracts is constant; when absent the extracts give a negative physiological result.

Moore did not succeed in separating or identifying it. It was not attacked by acids, or by boiling for a few minutes, but it is destroyed by alkalis, by oxidising agents, and by prolonged boiling. It is not precipitated by alcohol, saturation with ammonium sulphate, by mercuric chloride, potassio-mercuric iodide or tannic acid. It does not reduce Fehling's solution even after boiling with mineral acids nor does it form a crystalline compound with phenylhydrazine. It is not volatile either alone or with aqueous vapour. It dialyses freely through parchment paper, and the highly active dialysate is free from proteid. It is not affected by gastric digestion.

From these characters, largely negative, one can say what the substance is not, though one cannot say what it is. It is certainly not a proteid. An investigation of the proteids of the gland shows that they are of the globulin and nucleo-proteid variety; in fact like the proteids of other cellular organs (D. N. Nabarro).[1] It is certainly not a carbohydrate, or a fat, or an alkaloid. This excludes neurine and its related alkaloid choline.

The only other research bearing on the question that I can find is by Paul Manasse.[3] He states that it has long been known that the medullary substance of the suprarenal capsules contains a material which becomes dark brown with salts of chromic acid, unless the capsules have been previously hardened in alcohol. The substance is soluble in alcohol, and shows great similarities to the body named

[1] *Proc. physiol. Soc.*, p. xvii., 1895.

[2] Schäfer (*ibid.*, p. iii., 1894, contained in *Journ. of Physiol.*, vol. xvi.) finds that the physiological action of these alkaloids is quite different from that of suprarenal extracts.

[3] *Zeit. f. physiol. Chem.*, vol. xx., 478, 1895.

jecorin, first separated by Drechsel[1] from the liver. The actual method of isolation is too technical to be given here, but Manasse used practically the same process with the suprarenal. Baldi[2] has also separated jecorin not only from the liver but from the spleen, which next to the liver gives the highest yield, brain, and muscle. Jecorin resembles lecithin in many of its properties and seems to accompany lecithin pretty constantly (Baldi). The resemblance to lecithin noted by Drechsel is accentuated by the fact discovered by Manasse, that it yields choline, glycero-phosphoric acid, and fatty acids on decomposition. The carbohydrate obtainable from it, and to which it owes its reducing properties, appears by its characters to be most like dextrose. The power which jecorin possesses of reducing Fehling's solution seems to vary with the source from which it is derived (Baldi).

The substance obtained from the suprarenals, however, is not jecorin, though it has a general resemblance to it. The two bodies differ in some of their solubilities, and in the fact that the new substance does not reduce Fehling's solution until after prolonged boiling with sulphuric acid; the sugar then formed appears to be dextrose. If one next compares the percentage composition of the two substances the difference is seen to be very marked, especially in the amount of carbon, nitrogen, and phosphorus. This is shown in the following table.

	Jecorin.		Substance from suprarenals.
	Drechsel.	Baldi.	Manasse.
C	51·32-51·64	46·88-46·89	41·43
H	8·11- 8·25	7·81- 8·09	7·16
N	2·86	4·36- 4·88	0·3
S	1·42- 1·47	2·14- 2·70	1·8
P	2·2 - 3·7	2·29- 2·75	4·44
Na	2·72	5·72	—
O	30·10	—	—

[1] *Journ. prakt. Chem.*, xxxiii., 425.

[2] *Du Bois-Reymond's Archiv*, 1887, Suppl. No., p. 100.

Manasse's research appears to have been undertaken without any special reference to the question of the physiological action of the suprarenal capsules, or of the work of Schäfer and his colleagues. But it is by no means uninteresting to inquire whether his investigation does not after all give the key to the problem. Here is a substance, of a reducing nature, resembling jecorin it is true in some points, but practically unique, and confined to the medulla of the suprarenal capsules. It is soluble in alcohol, and Moore's substance is not; but the addition of a little water to the alcohol renders Moore's substance soluble. It is possible to explain this difference therefore by variations in the strength of the alcohol used. Another distinction between Manasse's and Moore's substance is that the former reduces Fehling's solution after *prolonged* treatment with sulphuric acid, whereas the latter does not. Perhaps this difference may be explained by different interpretations of the word prolonged. At any rate the matter appears promising, and all will watch with interest further developments of this remarkable series of investigations.

W. D. Halliburton.

THE CLASSIFICATION OF IGNEOUS ROCKS.

THERE is perhaps no branch of study in which the accumulation of material has so far outrun the advance of systematic science as in Petrology, and to the zeal thus shown on the purely descriptive side of the subject may be attributed in some measure the confusion so apparent in the classification and nomenclature of the igneous rocks. There are, however, in the nature of the subject itself, considerations which seem to make classification a matter of peculiar difficulty. Petrology knows no species or genera. The laws of precision, chemical and crystallographic, which give individuality to minerals, and lead to the grouping together of allied minerals, afford us no help in dealing with complex mineral aggregates such as igneous rocks. Neighbouring rock-types, and even widely diverse types, are linked together by an indefinite series of intermediate varieties, and are found in the field to graduate into one another by insensible transitions. Of any such fundamental principle as that of descent, which defines species and underlies classification in the organic world, petrologists have obtained as yet only a dim glimpse. It is perhaps in this direction that the solution of the problem is to be sought, but a clear conception of all the physical and chemical principles involved in the genesis of rocks from molten silicate-magmas is a necessary preliminary which is at present far from being realised. Meanwhile, it may be of interest to recall some of the steps by which the present position of the question has been reached.

To trace completely the growth of opinion with regard to the classification of rocks would be to write a history of Petrology. Moreover, the earlier writings on the subject, though full of interest historically, scarcely touch the science as now understood, and therefore need not engage our attention. Instead of following a chronological sequence it will be more instructive to consider briefly the several points of view of those who have approached the subject

from different sides, and we will therefore notice in turn the different factors which, from time to time, have been made to enter into schemes of rock-classification. The development of these schemes shows a progress from a purely artificial towards a natural system, the latter being, however, a goal sought rather than an end attained. The earlier workers seem to have held no conception of a natural system of classification, even as an object to be aimed at. Their view is perhaps not unfairly represented by the opening sentence of Pinkerton's preface in 1803: "Le seul avantage qui résulte de tout système méthodique en histoire naturelle, est de soulager la mémoire".[1]

The first factor that we shall notice in the various schemes of classification of igneous rocks is *geological age*. The rival systems in the early part of this century were often referred to as the "geological" and the "mineralogical". They were to some extent identified with Plutonism and Neptunism, but the two principles in rock-classification may be traced back to an earlier date. Linnæus, Wallerius, and others of the Swedish school in the middle of the last century, embraced the rock-types known to them in their mineralogical systems of the inorganic kingdom; but they had also concluded, from observations in their own country, that the several broad divisions of rocks occupied each its own definite position in the crust of the earth. In other words, they recognised a stratigraphical succession of rocks, which they believed to be of general application. This idea is more familiarly associated with the name of Werner, who elaborated it into a system based on his researches in the mountains of Saxony. In these early speculations, which were indeed the beginning of stratigraphical geology, the important conception of an ordered succession of rocks was pushed so far as to obscure the distinction between truly bedded sediments and igneous rock-masses; while, further, the succession observed in a single area was assumed to hold good universally. Werner's scheme of

[1] I have met with this earlier work of Pinkerton only in the French translation, *Esquisse d'une nouvelle classification de minéralogie*.

classification has at the present day no more than a historic interest; perhaps the best expositions of it are contained in the writings of some of the Scottish geologists, Jameson, MacCulloch, etc.

There are, however, certain relics of Wernerism still extant, two of which may be mentioned in passing. One is the theory, still held in various forms, that most of the crystalline schists and gneisses have been formed at a period of remote geological antiquity, and no similar rocks have been produced since the date of the "Urgneiss" or "Grundgebirge". The other is the doctrine of an essential distinction between the "older" and "younger" igneous rocks, on which some remarks have been made in an earlier paper in this journal.[1]

Apart from these ideas, which appear somewhat crude and arbitrary in the light of modern researches, various attempts have been made to incorporate the chronological principle in one form or another in classifications of igneous rocks based on wider generalisations. It has long been recognised that eruptions, whether deep-seated or superficial, have not been equally distributed over the geological time-scale. It is indeed the long pause in igneous activity in Mesozoic times that has lent colour to the doctrine of an older and a younger series of rocks already alluded to, and closer examination reveals the existence of at least three or four maxima of activity during the ages contemplated by stratigraphical geology. The correspondence, as regards both time and space, of igneous activity with the great crust-movements that have shaped the continental masses, and determined the dominant mountain ranges, is a subject upon which we cannot enlarge here, but some have thought to find in this a philosophical starting-point for a genetic grouping of igneous rocks. The recurrence of identical types at widely separated epochs, however, forbids the idea of a systematic classification on such a basis. If, to fix our conception, we imagine with Bertrand (24) that all the rocks belonging to any one period of crust-movement and

[1] "Science Progress," vol. ii., pp. 48-63, 1894.

igneous activity have been derived by processes of differentiation from one great subterranean magma-basin, we must admit that these processes have completely masked any original differences that may have subsisted between one parent magma and another.

There is another point of view from which the chronological sequence of igneous rocks may conceivably become an element in a natural classification, *viz.*, with regard to the order of eruption of different rock-types belonging to one period of activity, and within a given region. One of the first geologists to address himself to this question was Richthofen (6), whose "Natural System of Volcanic Rocks," worked out chiefly in the Western States of America and in Hungary, has been given a wide application. The rocks considered by the author were those of the Tertiary epoch, as developed in what he has styled "massive" eruptions. He divided them into five orders—propylites, andesites, trachytes, rhyolites, and basalts—each subdivided into families, and he laid down as a universal law that these several orders have been erupted in regular sequence as named. More extended knowledge has only partially confirmed this generalisation of Richthofen as regards the succession of the Tertiary lavas; nor do the rocks of other periods—*e.g.*, those of the Hercynian eruptions in Central Europe—appear to conform to any such simple law. The law that the extravasation of material of intermediate composition has normally preceded the appearance of either acid or basic lavas seems to be borne out in a large number of instances, and such a law connects itself simply with speculations on the differentiation of rock-magmas. But any classification built upon such considerations as this would evidently be founded upon genetic principles, the factor of relative age not being really an essential one.

We pass on to consider the classifications based upon descriptive characters, the foremost being, of course, *mineralogical constitution*. This being a necessary element in the description of rocks has naturally come to figure prominently in the definition and classification of rock-types, and has often been assigned the first place. The earlier writers

who adopted this view, whether separating composite rocks from simple minerals or not, were content, for the most part, to group the rocks in accordance with the dominant mineral of each. At that time there were, however, formidable obstacles in the way of any mineralogical classification of rocks. In the first place, the minerals themselves were very imperfectly known, and their mutual relations scarcely understood at all. A grouping of minerals according to their "external characters" could not lead to a rational classification of rocks upon a mineralogical basis. This difficulty naturally disappeared with the advance of mineralogy, especially with the importance attached by Haüy and his school to crystallography, and the chemical investigations of Berzelius and others. A more serious obstacle, in what we may term the pre-microscopic days of Petrology, was that the fine-textured aggregates and the compact ground-mass of many porphyritic rocks defied mineralogical analysis. The writers who, in the earlier decades of this century, advocated a mineralogical as opposed to the so-called geological system of classification, were accustomed to divide all rocks into homogeneous and heterogeneous, including under the former head such substances as felsite ("compact felspar") and basalt, which were not resolved by the methods then in use. Cordier's demonstration, in 1815, of the composite nature of the compact ground-mass sufficed to shake but not to destroy this distinction. Thus we find Brongniart, twelve years later, subdividing his "homogeneous" rocks into two orders—"phanérogènes" and "adélogènes"; the former being simple rocks consisting in each case of one known mineral; the latter being formed "wholly or in part by a mechanical union of mineral particles," which he makes no attempt to specify. Such rocks as basalt are included in this latter order, while even in the former we find, *e.g.*, lherzolite under the head of pyroxene.

The microscope has now almost completely removed the difficulties felt by the earlier petrologists, although the ground-mass of many acid rocks—the "petrosilex" which baffled Cordier—has furnished material for controversy down

to a very recent date. Microscopic investigation has also detected the important part played in various rocks by such minerals as nepheline, melilite, and the rhombic pyroxenes. With the more complete knowledge of the actual mineralogical constitution of igneous rocks thus gained, a comprehensive classification on this basis became for the first time possible, and this is accordingly a prominent element in most current systems. The application of such systems to the great variety of rocks now described and studied has, however, brought out other difficulties, which are evidently inherent in the method.

The choice of characteristic minerals has been determined, in the first place, by the important *rôle* filled by the felspar group in the great majority of igneous rocks; and, in the hands of Roth, Zirkel, Rosenbusch, and their followers, the dominant kind of felspar in a rock has come to be a prime factor in fixing its classificatory position. Here the need of some ready practical method of discriminating the several felspars has presented an obstacle which is only gradually being surmounted. The presence in some rocks of free silica in addition to acid felspars and the poverty or absence of any felspathic constituent in other rocks have been used to mark further divisions, while the coming in of the "felspathoid" minerals, leucite and nepheline, affords another character of the same rank. Excepting perhaps olivine, the ferromagnesian minerals have been reserved to mark subdivisions of a lower order.

With whatever degree of success such a scheme may represent the true relationships of rocks of granitoid structure, it can be applied only in a modified form, and not without caution, to the porphyritic types. This point has been emphasised by Michel Lévy (23) in an elaborate critique of the second edition of Rosenbusch's *Manual*. He maintains the propriety of taking as a starting-point in mineralogical classification the dominant "white element" (felspar or felspathoid) of the second period of consolidation of the rock, rather than the larger and more prominent crystals of the first period, which often make up but a small proportion of the total bulk. He claims to show that the

grouping arrived at by Rosenbusch is, in many respects, arbitrary, and fails to present a proper correspondence between the families of the porphyritic and the granitoid rocks; while the classification proposed by Fouqué and himself in 1879, which he reproduces with little modification, is much truer to nature (15). It may be remarked that the petrologists of the French school have endeavoured more systematically than the Germans to carry out the distinctions based on the different kinds of felspar, as, for example, in the separation which they make between andesites and labradorites, the one characterised by andesine, the other by labrador felspar. The common use by German and English petrographers of the name "plagioclase," covering such widely different minerals as albite and anorthite, has probably done much to retard the recognition of essential differences among rocks thus loosely thrown together; and we may endorse the hope expressed by Becke and Michel Lévy that methods of increased precision may drive this vague term out of geological literature. Meanwhile, the division into alkali-felspars and soda-lime-felspars, with such subdivision of the latter as is practicable, must be preferred to the distinction founded on crystallographic characters.

The classifications of igneous rocks in current use take account not only of the constituent minerals but of their mutual relations, *i.e.*, of the *structural characters* of the rocks. An arrangement constructed on this two-fold basis may indeed claim to give some approach to a natural system founded on genetic principles; since the mineralogical constitution of a rock represents in some degree the nature of the parent magma, while the structure affords some index of the conditions of consolidation. In the older systems structural features of comparatively trivial importance were often elevated to a prominent rank, and no distinction was observed between original and secondary characters; so that, for instance, spherulitic and amygdaloidal structures might be included together under the head of "variolite". Even in some modern text-books a strict fidelity to purely descriptive characters sometimes leads to anomalies from the genetic point of view.

Rosenbusch (16) has adopted an important distinction between the even-grained (*körnig*) structure, in which each constituent mineral belongs to a single stage in the consolidation, and the porphyritic, in which there is a recurrence of one or more minerals in a second generation, so that the constituents belong to two periods, the "intratelluric" and the "effusive". The two types of structure are typically exemplified in the plutonic and the volcanic rocks respectively. On this point the German and French schools are at variance, the latter claiming to recognise two distinct periods of consolidation in almost all igneous rocks,[1] but on examination the difference seems to be one of theoretical rather than practical importance. A more real difference of opinion exists as to the propriety of establishing an intermediate division between the plutonic and the volcanic types of structure. Such a division is roughly represented by the "dyke-rocks" of Rosenbusch (22, 36), who, however, does not carry out the principle systematically. The difficulty arises chiefly with reference to the basic rocks, the strong tendency to crystallisation of magmas poor in silica rendering the structural and textural characters in their case of less significance than in the more acid rocks. Brögger (35) has insisted upon the necessity of a division intermediate between the plutonic and the volcanic, which he terms "hypabyssal," the distinction being based practically upon structural characters; while Roth, Zirkel, and Lossen, as well as the French writers, have considered such a division superfluous and ill defined.

It is manifest that, if we are to work towards a true natural classification, we must have regard in the first place to characters which are original and essential. This brings us to another question on which unanimity has not yet been attained. Allport, Bonney, Rutley, and other English petrologists have long maintained that many fine-textured rocks, especially among those of acid composition, owe their present structure to the secondary devitrification of a mass

[1] For further criticism of Rosenbusch's views on this point see Zirkel (34), and especially Cross, *Fourteenth Ann. Rep. U.S. Geol. Surv.*, p. 233, 1895.

originally vitreous. Whether this idea has in some cases been pushed too far is a point that might be argued, but it is certain that the Continental workers as a body have not yet fully appreciated its importance. The extent to which such secondary changes may obscure the true nature of the rocks is well illustrated by the ancient lavas of the Eastern States of America. Recent papers, published in that country, have fully recognised the importance of devitrification and other transformations, and the altered rocks have been designated by such terms as aporhyolite, apodacite, etc., to mark at once their initial characters and the fact of their subsequent alteration.

There is a sense in which the *geological relations* or mode of occurrence of a rock may enter into petrological classification. On the one hand, as marking the conditions under which consolidation took place, the mode of occurrence is connected with genetic considerations; on the other hand it is in close relation with the structure and texture of the rock, as indeed is implied in the use of such terms as "plutonic" and "volcanic" to denote divisions of rocks which are really defined by structural characters.

We ought next to consider to what extent the *chemical composition* of igneous rocks may enter, directly or indirectly, into their systematic arrangement. It is clear that if we are at liberty to regard each rock as representing the result of consolidation of a given molten magma, the composition of that magma may be proposed with considerable cogency as a prime factor in petrological classification. The empirical laws which seem to hold with a high degree of generality in the chemical analyses of igneous rocks, pave the way for such a system. The percentages of the several oxides are found to vary within more or less defined limits, and their variations are in great measure correlated, so that with an increase in the amount of silica we find generally an increase in certain other oxides and a diminution in others. Thus a large proportion of igneous rocks fall roughly into a linear series, having at the "acid" end types rich in silica and alkalies, at the "basic" end others rich in magnesia and iron-oxides. The laws here roughly expressed, and subject

to important exceptions, presumably result from fundamental principles upon which we can at present only speculate ; but it seems safe to assert that they will be embodied in any natural system of classification which may ultimately be arrived at.

Since we cannot have a chemical analysis of every rock it is clear that the practical application of a chemical classification must be in general an indirect process. The object kept in view must be to construct such a system, based on mineralogical characters, as shall most closely correspond with the chemical grouping taken as a guide. The relatively small number of the common rock-forming minerals —a consequence of the chemical laws already alluded to— facilitates such a scheme, but there are obvious considerations which limit its application. In the first place the mineralogical analysis of a rock is necessarily a qualitative rather than a quantitative one. This difficulty is only very partially evaded by ranking some mineral constituents as "essential" and others as "accessory," or by trying roughly to take account of the relative proportions of the essential minerals in a rock. The implied discrepancy, however, between a rough mineralogical and an accurate chemical analysis is perhaps smaller than it appears. It may even be argued that, where the two estimates differ, a mineralogical is not less likely than a chemical characterisation to lead to a true interpretation of relationships, for in cases where the relative proportions of the minerals vary widely (*e.g.*, in gabbros) we often find the extreme types forming parts of one and the same body of rock.

A more difficult question arises in the case of the volcanic rocks, most of which contain more or less unindividualised glassy matter. A mineralogical description, however complete, cannot express the chemical composition of a rock which consists only partly of minerals. It may be said that the minerals which are actually found give an indication of the composition of the magma from which they have crystallised out ; but, granting this, can we safely assume that that magma had the same composition as the rock finally consolidated? The case has been well put by Judd

(26) in a paper dealing with the volcanic ejectamenta of Krakatoa. These belong to the hypersthene-andesite group, and are comparable with the ancient and modern lavas of Santorin, the rock of Buffalo Peaks in Colorado, and the freshest of the Cheviot lavas. All these consist of the same minerals in roughly the same relative proportions, with residual glass of approximately the same composition in all. Nevertheless these rocks range in total chemical composition from an acid type with 70 per cent. of silica to a basic type with 52 per cent. This variation is connected with the relative proportion of the acid glass to the total minerals (on the whole basic), this proportion being 9 : 1 and 1 : 9 in the extreme types mentioned. In the author's opinion these and other facts point to the conclusion that "after the partial separation of a magma into crystals and a colloid residue, the two may be separated by a process of liquation, and subsequently be mingled again in varying proportions". In a later paper on the intermediate lavas of the Western Islands of Scotland the same writer applies this idea to the associated rocks belonging to one volcanic centre, that of Ardnamurchan, and expresses the opinion that "there is no direct and necessary relation between the porphyritic crystals of a volcanic rock and the magma by which they are enveloped". Without endorsing the rather extreme conclusion we may note that we have in this suggestion a method of differentiation in rock-magmas demanding only mechanical means, and possibly capable of producing important results.

Apart from the question just raised, if we know the component minerals of a holocrystalline rock, and approximately the relative proportions in which they are present, we have a sufficient index of the place of the rock in any scheme founded upon total chemical composition. It is interesting to note that the converse does not hold; a bulk-analysis of a rock would not always enable us to predict its mineralogical constitution. For example, Iddings (33) has pointed out that some of the lamprophyres are identical in chemical composition with certain leucite-bearing and other lavas, although the two sets of rocks differ widely as regards

their constituent minerals. A difference, though smaller in degree, is noticeable in the mineralogical constitution of many plutonic rocks and the minor intrusions belonging to them. In other words, the minerals developed by the crystallisation of a given magma depend not only on the composition of the magma, but also on the conditions attending its consolidation. A classification founded primarily on chemical principles, therefore, will sometimes bring together rocks which in a mineralogical scheme would be separated. We may note in passing one conclusion to be drawn from such facts as those cited by Iddings—though it is not the conclusion drawn by that author—namely, the necessity of a "hypabyssal" division of rocks to receive such types as the lamprophyres.

A complete chemical grouping of igneous rocks would probably take account of the proportions, firstly of silica; secondly of the alkalies with reference to the silica, and of soda relatively to potash; thirdly of lime, magnesia, and the iron-oxides, relatively to one another. Having regard to these points, it would be possible to construct a system which would for the most part agree with a mineralogical one. Where the two differed, the chemical system would more accurately represent fundamental relationships of the rocks, regarded as consolidated rock-magmas.

Adopting the point of view just indicated, *viz.*, that the total chemical composition of an igneous rock is the prime datum, we may inquire what other character or circumstance is to be taken as a supplementary factor in such a scheme of classification. And we are led logically to the conclusion that the only remaining "independent variable" is the *ensemble* of conditions governing the consolidation of the magma supposed given, or in other words the geological environment, which is now represented for us by the mode of occurrence and geological relations of the rock. We have seen that this may influence, not only the structural and textural characters, but also the mineralogical constitution of a rock of given composition. The abyssal, hypabyssal, and superficial (volcanic) conditions afford a three-fold division, which is doubtless susceptible of further

refinement. These, and subdivisions of them, while sufficing for rough purposes, cannot, however, adequately represent the complex conditions through which many igneous rocks have passed during what we may describe as a varied life-history. The record preserved in the rocks themselves often gives evidence of successive stages of consolidation under different conditions, of minerals crystallised out at one stage and partially or wholly resorbed at another, and of other changes of circumstances, either gradual or abrupt.

The assumption that the total chemical composition of a partially fluid mass has remained constant through such vicissitudes as are thus indicated, becomes on reflection a very improbable one. Nor can we set aside the numerous cases adduced in late years to prove that chemical, as well as mineralogical differentiation has been brought about under suitable conditions in originally homogeneous rock-magmas. As regards the nature of these conditions, and the mode of operation of the differentiating processes, almost everything is still to be learnt ; but the facts seem to justify the belief that the bulk-analysis of an igneous rock, as well as its mineralogical and structural characters, may often be dependent in greater or less degree upon the conditions governing its consolidation, the circumstances attending its eruption, and other facts in its history. Light will be thrown upon the subject both by the study of individual rocks and by the comparison of groups of associated rocks to which we are warranted in attributing a community of origin.

The idea of relationship or "consanguinity" among igneous rocks, pointing to some kind of common origin, brings us to contemplate the possibility of a classification based on *genetic* considerations. This, as already remarked, can at present be regarded as only a distant goal, and to follow up the subject would lead us into untrodden paths. We shall content ourselves with noticing the work of Brögger (35), who has made a bold attempt to apply the principles of differentiation to a systematic arrangement of the numerous peculiar rock-types of the Christiania basin.

Rosenbusch (22), perhaps for convenience of descriptive treatment rather than as a strictly logical arrangement, divides the igneous rocks in the first place into three classes according to their mode of occurrence, a factor with which the micro-structure of the rocks is closely connected. These three classes, "Tiefengesteine," "Ganggesteine," and "Ergussgesteine," are then divided into families defined by mineralogical (and to some extent implicitly by chemical) characters. It is noticeable that while there is a fair correspondence between the families of the first and third classes, the "Ganggesteine" or "dyke-rocks" include some, such as the aplites and the lamprophyres, which are not easily paralleled among either the plutonic or the volcanic rocks.

Brögger, on the other hand, takes as his primary divisions families defined by chemical characters. These families may be divided into subfamilies, also on chemical grounds, and each includes abyssal, hypabyssal, and superficial rocks. The granite family, for example, embraces all igneous rocks having the composition of the granites; such as normal granites, granite-porphyries, granophyres, quartz-porphyries, pitchstones, rhyolites, etc. But it also finds places for rocks which, while having close relations with these, differ from them in chemical, as well as in mineralogical and structural characters. The hypabyssal members and marginal modifications (Grenzfaciesbildungen) fall indeed under two heads; non-differentiated ("aschiste") and differentiated ("diaschiste"). Under the former head are placed the quartz-porphyries, granophyres, etc., while the latter covers such rocks as minettes and aplites.

An important feature of Brögger's system is the conception of a *rock series*. In a tabular scheme the various types are ranged in rows and columns, the rows corresponding to families, and the columns to series. The members of one series thus belong to different families. The ideal rock series consists of a number of such types, connected by intermediate gradations, and falling under the same broad structural division (plutonic, hypabyssal, or volcanic). They have certain mineralogical and chemical characters in

common, while in other characters they present a continuous variation from one end of the series to the other. One such series, comprising the types grorudite, sölvsbergite, and tinguaite, is described in detail in the first instalment of the author's "Eruptivgesteine des Kristianiagebietes". These are hypabyssal rocks occurring as dykes. They have in common the constantly low percentages of lime and magnesia, the preponderance of soda over potash, etc.; while the proportions of other constituents vary considerably, though in a regular manner, the percentage of silica falling from seventy-four to fifty-six, that of alumina rising from eight to twenty, and that of the alkalies jointly from eight to sixteen.

The types just mentioned belong to different families, which are also represented in the district both by plutonic and by other hypabyssal series. Thus the grorudite belongs to the family (or rather subfamily) of soda-granite. It agrees with this plutonic rock somewhat closely in chemical composition, but is rather richer in ferric oxide and alumina. It is therefore to be referred to the "dia-schiste," but does not imply an extreme differentiation as compared with the soda-granite. Sölvsbergite bears a like relation to the quartz-syenite of the district (nordmarkite), and tinguaite to the nepheline-syenite (laurdalite). The three dyke-rocks thus seem to have had similar histories and to play corresponding parts in the great group of associated igneous rocks of the district. Another series includes the rhomb-porphyries, some of which are well-known rocks. These have special characters in common, such as the peculiar crystallographic habit of the felspar crystals, which distinguishes them at once from the other hypabyssal series; but they embrace a considerable range of types belonging to different families. The plagioclase-rhomb-porphyry corresponds to the plutonic gabbro-diabase, the ordinary type to the augite-syenite (laurvikite), the nepheline-bearing type to the laurdalite, and the quartz-bearing to the nordmarkite.

Contrasted with the idea of a rock series is what the Norwegian geologist terms a "Faciessuite," comprising a

graduated succession of types which may form parts of a single body of eruptive rock, such as a laccolite. The variations, chemical, mineralogical, and structural, in such a suite are the result of differentiation *in the laccolite itself;* while the differences between the several members of a series are due to "*tiefmagmatische*" differentiation, effected in the parent magma-basin prior to the intrusion.

In the case of such an assemblage of igneous rocks as those of the Christiania district, with so strongly marked consanguinity, it can scarcely be doubted that all have originated, by differentiation of more than one order, from a common stock. If similar rocks in other regions have had a like origin, a complete knowledge of the mutual relations of the rocks should lead to a natural classification founded on fixed genetic principles.

The condition here expressed seems to be a necessary one. It is, of course, no objection to such a system as is here contemplated that it brings under one family rocks which differ widely in composition as well as in characters; but if one rock-type may figure under two or more families, the system seems to be lacking in simplicity. Brögger has remarked, for example, how bostonite and camptonite may arise as complementary products of differentiation of either a gabbro- or a nepheline-syenite-magma; so that, in a genetic grouping of general application, the camptonites of Norway and of New England would fall into two widely separated families. If the facts have been rightly interpreted, such anomalies must be inseparable from a general classification founded on genetic relationships. In any case, such work as Brögger's, involving minute comparison of many associated rock-types with reference to their mode of origin, cannot fail to throw light upon the problem of a natural classification.

In the foregoing pages no idea has been given of the details of the various classificatory schemes advocated by different petrologists. This can be obtained from the works cited below. They are selected from a rather voluminous literature to represent the different ways in which the subject has been approached, and in some measure the develop-

ment of opinion since chemical and microscopical researches have afforded data for attempts in the direction of a natural system.

No allusion has been made above to the question of the *nomenclature* of rocks. This is to some extent affected by the difficulties which beset the problem of rock-classification, and indeed many of the names now in use are little better than "Sacknamen," as Brögger has expressively described them. Names covering groups and families of rocks cannot become fixed until the groups and families themselves have been accurately defined, and their limits have become a matter of common agreement. Much confusion has been gratuitously introduced by the practice of arbitrarily extending, restricting, or changing the meaning of classificatory or descriptive names. Some, like syenite and granophyre, are used in a sense which contradicts their etymology; others, like pyroxenite, are applied by different writers to totally different groups of rocks. One fruitful source of confusion is the extension to a family of rocks of a name, such as foyaite or tonalite, belonging originally to a single type.

Names of rock-types must clearly be numerous, and increasingly so. To this there can be no objection, provided the names are strictly reserved for rocks of the types in question, and provided a new name is given to a new type of rock only when the latter has been fully described. The names least likely to cause difficulty in the future are those derived from the localities of the typical rocks, and thus embodying information which is independent of any theory. To take the type rock from one locality and the name from another, as in the monchiquite of Hunter and Rosenbusch, is a proceeding to be deprecated. If ever a satisfactory classification is arrived at, it will not be difficult to embody it in a new nomenclature, possibly a binomial or trinomial system as suggested by Wadsworth (17); but any such systematic terminology presupposes a knowledge of the true relationships of igneous rocks which is still a desideratum.

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(12) LASAULX, A. VON. *Elemente der Petrographie.* 8vo. Bonn, 1875. [An example of a classification grounded on structural and textural characters.]

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ALFRED HARKER.

PAWLOW'S RESEARCHES ON THE PHYSIOLOGY OF SECRETION.

THE researches of Pawlow, of which I wish to give a short account in the following pages, are worthy of attention, not only by reason of their importance in filling a much-felt void in our physiological knowledge, but also because they embody a new departure in physiological method.

It might have been thought, when Ludwig, in 1851, discovered the innervation of the salivary glands, and showed that, by stimulation of certain nerves, a secretion of saliva could be invariably invoked, that our knowledge of the innervation of the other glands of the body would rapidly have been completed. Yet up to the date of Pawlow's researches the two most important secretions which are poured into the alimentary canal, *i.e.*, the pancreatic and gastric juices, although the subjects of numerous investigations, had foiled all attempts to discover the nervous impulses which control their production.

This want of success of the various experimenters is not to be wondered at when we consider the conditions under which so-called "physiological" experiments are usually carried out. It is a matter of common experience that the digestion of food, *i.e.*, the secretion of active digestive juices, is intimately dependent on the well-being, mental or physical, of the animal, and is absolutely checked by abnormal conditions, such as fright, pain, administration of narcotics (opium, chloroform, alcohol, etc.). Now in most experiments the dog or other animal employed is first poisoned with a large dose of morphia, is rendered fully anæsthetic with chloroform or ether, and in many cases is also poisoned with curare. If anæsthetics are omitted the animal is tied down and subjected to an extremely painful operation. It is under circumstances such as these that experiments on the innervation of the gastric and pancreatic glands have failed to have any definite results.

One other difficulty in experimenting on these glands

lies in the fact that the nerves supplying them (vagi and splanchnics) exert a powerful influence on the vascular system—an influence which might presumably affect or check altogether any secreto-motor functions that these nerves might possess.

Pawlow has succeeded in avoiding both these sources of error, and in proving conclusively the existence of definite secretory nerves to both stomach and pancreas. I will deal first with his researches on the pancreas.

Claude Bernard, in 1856, showed that a secretion of pancreatic juice might be evoked by introduction of ether into the stomach, and that a secretion already in progress was checked if vomiting occurred, both facts pointing to the existence of a distinct reflex nervous mechanism presiding over the secretion. This conclusion was further strengthened by the experiments of Bernstein (1869), who showed that the pancreas might be reflexly affected through the vagus (afferent) in two ways, and therefore concluded that this gland was provided with two sets of nerves, excitatory and inhibitory. Attempts, however, to find the efferent channels of these impulses failed.

In 1875 Heidenhain pointed out that direct excitation of the medulla, under certain circumstances, might call forth a pancreatic secretion, but failed, like his predecessors, in distinguishing the efferent nerves carrying the secreto-motor impulses.

Later on (1877) Pawlow and Afanassiew showed that excitation of the central end of all sensory nerves, and not only of the vagus, inhibited an already existing flow of pancreatic juice.

A consideration of these various experiments enables us to perceive certain precautions which would have to be taken in any subsequent experiments on the subject. There must be no excitation of sensory nerves in carrying out the preliminary part of the experiment. On the other hand, it is not permissible to make use of drugs or anæsthetics to depress the sensibility. In exciting the vagus steps must be taken to prevent simultaneous excitation of the cardio-inhibitory fibres.

Pawlow has pointed out two methods by which these aims may be accomplished. In the first method a permanent pancreatic fistula is established, and at the same time one vagus is exposed in the neck, ligatured and divided. The ligature is left hanging from the wound, so that at any subsequent period the nerve may be drawn out and stimulated without any discomfort to the animal. Three days afterwards, without any preliminary anæsthesia, the cut vagus is stimulated with any ordinary faradic current. The invariable result of the stimulation is a secretion of pancreatic juice which flows from the fistula and can be collected in the usual way. The latent period intervening between stimulation and secretion varies between fifteen seconds to two or three minutes. No effect is produced on the cardio-vascular system, the inhibitory nerves having degenerated in consequence of the section. Injection of atropin stops secretion, and prevents the effect of stimulating the vagus.

In the second method, the preliminary operation is dispensed with. In this case it is of course necessary to prevent the inhibitory impulses, which are started by the operative stimulation of sensory nerves, from reaching the gland. To this end, Pawlow, in the unanæsthetised animal, rapidly performs tracheotomy and division of the spinal cord just below the medulla, these two operations in his hands not occupying more than half a minute. The impulses which inhibit secretion seem to act reflexly through the medulla, so that they are in this way cut off from the gland. He then resects three or four ribs in order to expose the vagi below the point where they give off their cardiac branches, and finally makes a temporary pancreatic fistula. Stimulation of the vagi below the heart now gives rise to a secretion or increases the secretion already present. Using this method it is better to stimulate the vagi mechanically by means of Heidenhain's tetanomotor or to use single induction shocks at intervals of one second, instead of the faradic current.

The reason for this superiority of single shocks over the faradic current possibly lies in the fact that the vagus may include vaso-constrictor fibres to the pancreas. Pawlow

shows in his paper that the activity of the pancreas is largely dependent on its blood supply—a temporary anæmia of the gland induced by obstruction of the aorta or by stimulation of the splanchnics causing a total suppression of pancreatic activity, which may last several hours.

The second paper on pancreatic innervation, by Mett, deals chiefly with the influence of the vagus on the production of the pancreatic ferments and especially of trypsin. For the determination of the comparative proteolytic powers of the various specimens of pancreatic juice, he employs a new and simple method which presents considerable advantages over the old carmine-fibrin method of Grützner. Capillary glass tubes of 1 mm. bore are filled with egg albumen, which is then coagulated by insertion for five minutes into water at 95° C. They are then cut by a special machine into lengths of 1 cm. Two of these small tubes are placed in each sample of fluid, whose proteolytic powers it is desired to determine, and the whole is kept for seven hours at 40° C. The amount of albumen remaining at the end of the time is read off by means of a micrometer, and the digestive powers expressed by the number of mm. that have been digested. Thus, if the column of undigested albumen was three mm. in length, the digestive power of the fluid would be represented by seven.

In the first place, Mett shows that the pancreatic juice obtained by stimulating the vagus is active on all three classes of food stuffs, and can therefore be regarded as normal. It is an interesting fact that the pancreatic juice obtained in this way from fasting animals is actively proteolytic, whereas it is well known that a fresh extract of the same glands would contain no trypsin. In the second place, in cases where there is already a secretion of pancreatic juice on stimulation of the vagus, we get, not only increased secretion of fluid, but also increased formation of the ferment, so that the juice obtained after stimulation of the vagus is much more active than that obtained before. It is evident then that the vagus contains, to use Heidenhain's nomenclature, not only secretory but also trophic fibres to the pancreas.

In the third paper, by Kudrewetzky, the most interesting results refer to the action of the sympathetic and splanchnics on the pancreas. I have already mentioned that ordinary faradic stimulation of the splanchnics causes anæmia of the gland and consequent cessation of its activity. It is well known, however, that if we stimulate a "mixed nerve" by single induction shocks or mechanical means, the vaso-constrictor effects are little marked or absent. If these modes of stimulation be applied to the splanchnics or to the thoracic sympathetic, a secretion of pancreatic juice is evoked together with an increased formation of trypsin. The same effect may be produced with ordinary faradic stimulation if the splanchnics be divided six days previously so as to allow time for the vaso-constrictor fibres to degenerate. Kudrewetzky concludes that the splanchnics, as well as the vagi, contain secreto-motor and tryptogenic or trophic fibres to the pancreas.

THE INNERVATION OF THE GASTRIC GLANDS.

Still more striking and interesting are Pawlow's researches on the innervation of the stomach. In this case, as in the case of the pancreas, previous researches had added but little to our positive knowledge of the subject. As long ago as 1852, Bidder and Schmidt showed that in a dog with gastric fistula, the sight of food caused a secretion of gastric juice. In 1878 Richet published a series of interesting observations on a man with complete occlusion of the œsophagus and a gastric fistula. In this patient the mastication of sour or sweet things caused a copious secretion of gastric juice. Hence there could be no doubt that the nervous system has control over the gastric glands, and that some nerves exist carrying secreto-motor impulses to the stomach. Attempts to distinguish these nerve channels were however without success.

In experimenting on this point it is necessary that the animal should be in as normal a condition as possible, and that all escape of the saliva through the œsophagus into the stomach should be prevented. Pawlow carried out, therefore, the following operation. He first made a gastric

fistula, and a few days later exposed the œsophagus in the neck, divided it, and sewed the two cut ends to the two corners of the wound in the neck. The animal was fed by pouring milk or broth through the lower end of the œsophagus, or by the direct introduction of meat into the stomach. Any food taken by the mouth or any saliva secreted in the mouth and swallowed fell out of the opening of the œsophagus in the neck, and so was perfectly shut off from the stomach. After the operation the dogs diminished in weight for a few days, and then rapidly accommodated themselves to the altered mode of feeding, and increased in weight and remained in excellent condition for many months. In such animals it was easy to bring about reflexly a copious secretion of gastric juice. To this end it is only necessary to starve the animal for nine hours, and then give it a meal of meat to eat. The dog devours the meat with avidity, masticates it, but none of the meat reaches the stomach since it all falls out of the opening of the neck. The dog's appetite remaining therefore unappeased, he will go on eating for a very long time. The effect of this sham feeding is to call forth a reflex secretion of gastric juice. Between the feeding and the secretion a long, constant, latent period of five to six minutes intervenes. I may here quote one experiment to show the pronounced effect on the activity of the gastric gland, which is thus produced :—

Time.	Gastric juice secreted in five minutes.	
12·30	4·0 ccm.	Gastric juice contains a considerable amount of mucus.
12·35	3·0 ,,	
12·40	3·0 ,,	
12·45	2·4 ,,	

At this point the dog begins to eat meat.

12·50	1·8 ccm.	The gastric juice is almost completely free from mucus.
12·55	10·8 ,,	
1·0	15·4 ,,	
1·5	17·8 ,,	
Feeding stopped.		
1·10	16·0 ccm.	
1·15	12·0 ,,	
1·20	10·8 ,,	

This reflex secretion only occurred when meat was eaten and not when water, milk, or soup was given to the dog. It is interesting to note that in many cases merely showing the meat to the hungry dog would cause a secretion of gastric juice, and that this secretion stopped as soon as the dog recognised that he was being teased, and was not intended to have the meat—an instance of the influence of psychical events on the processes of digestion, of which we have so many examples in our daily life.

Here at last we have a means of obtaining a perfectly pure gastric juice free from all admixtures with foreign substances, such as food or saliva. The characters of this pure gastric juice have been investigated more nearly by Pawlow's pupil, Schoumow-Simanowsky. When secreted it is perfectly clear and colourless, its sp. gr. varying from 1003 to 10,059. It is feebly dextrorotatory, gives no biuret reaction, but gives the ordinary reactions (xantho-proteic, etc.) for proteids. It inverts cane-sugar and is strongly proteolytic. Its total solids vary from ·292 to ·60 per cent.; ash from ·10 to ·160 per cent. It is strongly acid, the acid varying between ·46 and ·58 per cent. HCl.

An extremely interesting point about this juice is that, when cooled to 0°, it deposits a fine powdery precipitate which appears to consist of pure pepsin. When this precipitate has fallen to the bottom, it is found that the acidity of the fluid increases from the top to the bottom. Thus in one case the upper layer contained ·45 per cent. HCl, while a sample from the lower layer contained ·63 per cent. HCl. In another case, the upper layer contained ·4 per cent., the middle ·45 per cent., and the lower layer 1·1 per cent. HCl. This increase of acidity in the lower layers shows that the acid must be in a condition of loose combination with the pepsin, and is therefore carried down with this body when it is precipitated by means of cold.

By treating large amounts of gastric juice, it is possible to obtain sufficient of this cold precipitate for analysis. The precipitate forms an extremely light powder which gives all the proteid reactions. Even after thorough washing it presents a markedly acid reaction, and in solution gives the tests

for free hydrochloric acid. It is easily soluble in water or glycerine. The watery solution gives a pp. on cooling to 0° C. and coagulates on heating to 60° C. It digests proteids readily. The active body, or pepsin, can also be obtained from gastric juice by saturation with ammonium sulphate, as Kühne has already pointed out. The pepsin obtained in this way agrees very closely with that obtained on cooling the gastric juice, as is shown by the subjoined elementary analysis of two samples of pepsin obtained in these two different ways.

	Pepsin Precipitated by Cold.	Pepsin Precipitated by $Am_2 SO_4$.
Carbon, -	50·73 per cent.	50·37 per cent.
Hydrogen,	7·23 „	6·88 „
Chlorine, -	1·17 and 1·01 „	0·89 and 0·89 „
Sulphur, -	0·98 „	1·35 and 1·34 „
Nitrogen, -	—— „	14·55 and 15·0 „

We have now to inquire what is the path of the reflex from the nervous centres to the stomach. If both splanchnics be divided in one of the dogs with gastric and œsophageal fistulæ, the dog after an attack of diarrhœa returns to a practically normal condition. Under these circumstances, it is found that the sham feeding is followed, as in the case of normal dogs, by a copious secretion of gastric juice. It is evident that the splanchnics do not carry the efferent impulses of the reflex act. The next step was to divide both vagi, and here certain precautions were necessary. Division of both vagi in the neck causes paralysis of the larynx and œsophagus, and death after two or three days from pneumonia. To avoid this catastrophe, Pawlow proceeded in the following manner: On one day in the anæsthetised animal, the gastric fistula was instituted, and then the right vagus exposed low down in the neck by an incision outside the sternomastoid, and divided below the subclavian artery, *i.e.*, below the point where it gives off its recurrent branch. The operations were of course performed with strict aseptic precautions, and the animals recovered rapidly. When they were quite well the double œsophageal fistula was made. A few days later, after recovery from the second operation, the animal was placed in a special holder made

for the purpose, and arrangements made for collecting gastric juice. A sham feeding was followed by the usual results. The left vagus was then rapidly exposed and divided without anæsthesia, and meat again presented to the dog. He devoured it as eagerly as before, but now not a single drop of gastric juice was secreted. After this double division of the vagi, the animal remained in fair condition for three of four days, but during the whole of this time the reflex secretion was absolutely abolished, showing that the vagi carry the secretory impulses from the central nervous system to the stomach. At the end of this time digestive troubles supervene. The food remains in the stomach and undergoes putrefaction in consequence of the absence of the normal acid secretion. The dog wastes rapidly and dies of a species of septicæmia, or of exhaustion consequent on continual vomiting. This mode of death, in consequence of division of both vagi, has also been described by O. Frey.

In all cases where we would study the functions of a nerve or nerve centre, we have two experimental criteria to guide us, *i.e.*, abolition of function or paralysis in consequence of division of tracts, induced or increased functional activity in consequence of excitation. An application of the second method in the case under discussion confirms the conclusion derived from the effects of section of the vagi. Here, as in dealing with the pancreas, we must take steps to avoid the usual cardio-inhibitory effects of stimulating the vagus. This aim can be attained by dividing the left vagus twenty-four hours before the excitation. I have already mentioned that after this double division of the vagus, the animal remains in a fairly normal condition for two or three days, although there is no secretion of gastric juice. If, however, twenty-four hours after division of the second vagus, the nerve be drawn out of the wound and stimulated, a secretion of gastric juice is the invariable result. Here, as in the case of the reflex secretion, there is a constant latent period of five to six minutes intervening between the application of the stimulus and the appearance of the first drop of secretion, showing that the delay is due to processes having their seat in the gastric mucous membrane itself.

It is better, for the reasons already given, to use rhythmically repeated induction shocks of one a second than the ordinary faradic current.

We must conclude that the vagus carries the secretory fibres both to the gastric glands and to the pancreas.

Although this conclusion is a most important acquisition to physiological science, yet I think still greater value must be attached to the methods employed in arriving at it. I cannot do better than quote Pawlow's own account of the causes which had led him to a successful solution of the problem. "It is clear that the ordinary traditional mode of carrying out a physiological experiment in which the animals are subjected to the influence of the various poisons, as well as to severe operations at the time of the experiment, involves dangers which are not sufficiently appreciated by physiologists. Many physiological phenomena may in consequence of these conditions be absolutely abolished, or, if observed, may present marked deviations from the normal. There is no doubt that these methods have rendered most excellent service. Yet it seems probable that physiology is fast approaching the time when it must seek for new experimental methods—methods in which our chief aim must be to diminish as far as possible the deviation of the animal from the normal." We have, in fact, to introduce physiological methods into physiological research.

Although they are not directly connected with the question of the innervation of the glands, yet the researches of Simanowsky, in conjunction with Professor Nencki, on the urine of animals, in whom the gastric juice is allowed to leave the body, are worthy of notice. As might be expected, the effect of this loss of gastric juice is to make the urine strongly alkaline and turbid with phosphates. On adding acid to the urine, it effervesces from the presence of alkaline carbonates. The urine is free from albumen or sugar. Chlorides are entirely absent, while the potassium and sodium salts are largely increased. The latter, in fact, may be increased to ten times its normal amount. This fact shows that, as suggested by Maly, the hydrochloric acid of the gastric juice is derived from dissociation of

the sodium chloride in the blood. Under normal circumstances the hydrochloric acid is reabsorbed from the alimentary canal with the reformation of sodium chloride. In Pawlow's dogs, however, the hydrochloric acid being allowed to flow away through the fistula, the soda set free in the blood had to be excreted by the kidneys.

Since the sodium chloride is split up to form the hydrochloric acid of the gastric juice, it seemed worth while to find out what would happen if the Na Cl in the blood were replaced by Na I or Na Br. Dogs which had been operated on in the usual way were fed on meat, milk, and bread which had been baked without the addition of salt. To their diet was added first two grammes and then six grammes of sodium iodide or sodium bromide as might be desired. In the experiments, in which sodium bromide was added, the dogs became, after fourteen days, so ill and sleepy that the experiment had to be stopped, and one dog died. Sodium iodide, however, did not seem to affect the dogs in the slightest respect. The dose could be increased far above the amounts mentioned above without producing any ill effects. What is the cause of this difference between the two drugs? The examination of the urine and gastric juice in the two sets of experiments at once threw light on the cause of this divergence. In the dogs which were fed with sodium bromide, the gastric juice was secreted normally and its acidity was slightly increased, but the acidity was due largely to the presence of free hydrobromic acid. In one sample more than one half the total acidity was due to hydrobromic acid. In this way a large amount of the bromide taken in was eliminated. A certain amount was also got rid of in the urine, and it is worth noting that the excretion of bromide in the urine continued for many days after the administration of the bromide had been discontinued. On the other hand, in the dogs fed with sodium iodide the gastric juice contained only minute traces of hydriodic acid or of iodide, the chlorides present being sufficient to account for the total acidity. In these animals practically the whole of the iodide was eliminated by the kidneys, the elimination being complete within two or three days after discontinuing the drug.

We see, then, that these two closely related classes of compounds, iodides and bromides, are fundamentally different in their behaviour with relation to the dog's organism. The bromides are taken up by the cells, where they can replace the chlorides, and in the cells of the gastric glands they are split up into free acid and alkali, just as are the chlorides. Since they are thus built up into the living framework of the body, their elimination is slow. The fact, however, that dogs die under their administration shows that the representation is not complete, and that the animal cell cannot for all its purposes utilise the bromides.

The iodides, on the other hand, cannot replace the chlorides, and are probably only taken up by the cells of the body to a very small degree. They pass through the body in solution in its juices, leaving its cells and their well-being unaffected, and are rapidly excreted in the urine. These few experiments seem to offer us prospects of a further insight into the chemical affinities of living protoplasm, and it is to be hoped that they are merely the first instalment of a long series of similar comparative experiments on the reaction of the living body to allied chemical substances.

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E. H. STARLING.

APPENDIX I.

NOTICES OF BOOKS.

A Text-book of Physiology. By M. Foster, M.A., M.D., LL.D., F.R.S., Professor of Physiology in the University of Cambridge. Sixth Edition. Part ii. comprising Book ii. The Tissues of Chemical Action with their Respective Mechanisms. Nutrition. London : Macmillan & Co., 1895.

The changes which have been made in the present edition of this text-book are merely such as to bring it up to date. But the book is slightly larger than its predecessor and is greatly improved by the addition of a separate index.

One is, however, tempted to ask whether still further improvements might not be made. The book has now been before the public sufficiently long to make it possible to form some idea of the class of men who use it, and as to whether it is perfectly adapted to their requirements.

So far as the ordinary medical student is concerned the unanimous opinion will be that it is too large, *i.e.*, it contains more than such men really want; moreover, there is always the very greatest difficulty in getting such men to read even a one-volume book properly, to say nothing of one of four or five volumes. And, indeed, the treatment of the whole subject is not such as is best suited to the medical student's needs; it is too much in essay form, and the facts are not brought out with sufficient prominence; so that although after reading up a subject in it a man could write a good essay on the special branch of physiology treated of, he would not be able to produce the answers expected in such an examination as that conducted by the Conjoint Board. For such men a book in itself less perfect, but with the facts more clearly stated and theories less fully set forth, is better adapted.

The men for whose wants Foster's *Physiology* is particularly well adapted are those who are reading for University examinations or for the higher examinations in London, such as the Fellowship of the Royal College of Surgeons. For these men are always students with abilities somewhat above the average, who have decided to devote sufficient time to physiology to gain a good general idea of the subject, and they find that a book to be useful to them must be full, and special attention must be paid to the theories held on the various points. But upon many subjects opposite theories are held, and it is surely well that these students should have some idea of the men who originated such theories, and should be able in certain cases to apply to the original papers for fuller information than any text-book affords, in order that they may be able to form their own opinion on the subject. Already there is far too much veneration for "authority" in science, especially amongst the younger generation, and it is rare to find even an advanced student who has really formed an opinion for himself after looking up the more important papers on any given subject.

Dr. Foster's text-book fails here : it is almost without names, and without references to original papers. We are aware that the plan of excluding such references has been adopted after much and careful consideration, but as the book has increased so much in size since the first edition was published in 1876 it surely appeals to a different class of students, and it is in the interests of these men that we venture to suggest a change. Admirable as far as it goes, it yet does not go far enough for our best students, for, as Dr. Waller points out, it is of great importance that these men should be made familiar with the "growing edge" of the science. We are exceedingly glad to notice that Dr. A. Sheridan Lea has been able to help Dr. Foster in the preparation of the present edition.

A Primer of Evolution. By Edward Clodd. London : Longmans, Green, & Co., 1895.

The question which immediately occurs to us in glancing through this book is, "Why was it written?" It is difficult to believe that it will serve any useful purpose, for all the information it contains is to be found in more convenient form and certainly accompanied by far better figures in other books equally accessible to the average student.

We fear Mr. Clodd's book will scarcely help to popularise a knowledge of evolution.

APPENDIX II.

ENUMERATION OF TITLES OF CHEMICAL PAPERS APPEARING IN JULY, 1895.

Vol. lx. No. 243. *The Philosophical Magazine and Journal of Science.* (August, 1895.)

Buchanan, F., The Use of the Globe in the Study of Crystallography (pp. 153-172). *Kuenen, Dr.*, Condensation and Critical Phenomena of Mixtures of Ethane and Nitrous Oxide (pp. 173-194). *Olszewski, K.*, Determination of the Critical and the Boiling Temperature of Hydrogen (pp. 202-210). *Wanklyn, J. A.*, and *Cooper, W. J.*, Fractional Distillation applied to American Petroleum and Russian Kerosene (pp. 225-228).

Vol. lviii. No. 348. *Proceedings of the Royal Society.*

Ramsay, W., A Gas showing the Spectrum of Helium, the Reputed Cause of D_3, one of the Lines in the Coronal Spectrum (pp. 65-67). *Lockyer, J. N.*, On the new Gas obtained from Uraninite (pp. 67-70). *Fraser* and *Tillie*, *Acokanthera Schimperi*, Its Natural History, Chemistry, and Pharmacology (pp. 70-73).

Tome 5. *Annales de Chimie et de Physique.* (August, 1895.)

Camichel, C., Étude experimental sur l'absorption de la lumière par les cristaux (pp. 433-493). *Berthelot*, Memoire sur la Thermochimie de carbures d'hydrogène (pp. 493-567). *Moissan* et *Gautier*, Nouvelle méthode pour la determination de la densité des gaz (pp. 568-573). *Berthelot*, Sur la fusion de la Silice pendant la combustion explosive de la dynamite (pp. 573-574).

Tomes xiii.-xiv. No. 14. *Bulletin de la Société Chimique de Paris.* (20th July, 1895.)

Cazeneuve et *Haddon*, Les causes de la coloration et de la coagulation du lait par la chaleur, formation de l'acide formique aux dépens de la lactose (pp. 737-740). *Desgrez*, Nouvelle synthèse de quelques nitriles aromatiques (pp. 735-737). *Tanret, C.*, Sur les modifications moleculaires de glucose (pp. 728-735). *Tassillo*, Sur quelques combinaisons halogénées basiques des metaux alcalino-terreux (pp. 725-727). *Thomas, V.*, Action du peroxyde d'azote sur les sels halogénées d'antimonie (pp. 722-725). *Sisley, P.*, Sur le pouvoir tinctorial des phénols pour les mordants ferriques (pp. 740-743).

Tomes xiii.-xiv. No. 15. (5th August, 1895.)

Bouchardat et *Tardy*, Sur les alcools derives de l'eucalyptene (pp. 765-769). *Dupont, J.*, Recherche de l'huile de coton dans les Saindoux Americains (pp. 775-780). *Guerbet, M.*, Sur l'acide isocampholique (pp. 769-775). *Jean, F.*, Analyse du Saindoux et des grasses similaires. Recherche des huiles vegetales (p. 780). *Massol, G.*, Sur les points de fusion et de solidification de quelques acides de la serie grasse (pp. 758-759). *Varet*, Récherches sur sels mercurieux (pp. 759-765).

Tome cxxi. No. 2. *Comptes Rendus hebdomadaires de l'Académie des Sciences.* (8th July, 1895.)

Grimaux, E., Action du chlorure de zinc sur la résorcine (pp. 88-91). *Lecoq de Boisbaudran*, Volumes des sels dans leurs dissolutions aqueuses (pp. 100-102). *Haller* et *Guyot*, Sur la diphenylanthrone (pp. 102-106). *Hubault, P.*, L'existence de phosphore en proportion notable dans les Huitres (p. 111). *Duboin, A.*, Quelques méthodes de reproduction des fluorures doubles et des silicates doubles formés par la potasse avec les bases (p. 111). *Duboin, A.*, Analyse de la leucite et de la néphéline purement potassique (p. 111). *Pallas*, Surpression dans les minés de houible (p. 112).

Tome cxxi. No. 3. (15th July, 1895.)

Carnot, A., Sur un gisement de phosphates d'alumine et de potasse trouvé en Algerie, et sur la genèse de ces mineraux (pp. 151-155).

Tome cxxi. No. 4. (22nd July, 1895.)

Girard, A., et *Lindel, L.*, Récherches sur la composition des raisins des principaux cépages de France (pp. 182-187). *Raoult*, Sur les phénomènes osmotiques qui se produisent entre l'éther et l'alcool méthylique, à travers differents diaphragmes (pp. 187-189). *Haller, A.*, Action de l'isocyanate de phényle sur quelques acides et éthers (pp. 189-193). *Gaston, S.*, Un phénomène de phosphorescence obtenu dans les tubes contenant de l'azote raréfié après le passage de la décharge électrique (pp. 198-199). *Mourlot, A.*, Sur le sulfure de manganèse anhydre cristallisé (pp. 202-204). *Thomas, V.*, Sur quelques propriétés des combinaisons de chlorure ferreux et de bioxyde d'azote (pp. 204-206). *Hugot, C.*, Sur quelques phosphures alcalins (pp. 206-208). *Massol* et *Guillot*, Chaleurs specifiques des acides formique et acetique surfondus. Modifications à apporter an thermocalorimètre de Regnault, en vue de la déterminàtion de chaleurs spécifiques d'un grand nombre de liquides surfondus (pp. 208-210). *Henry, L.*, Formation synthetique d'alcools nitrés (pp. 210-213). *Behal, A.*, Oxydation de l'acide campholenique inactif (pp. 213-216). *Fleurent, E.*, Constitution des matières albuminoïdes végétales (pp. 216-219). *Maze, l'Abbé*, Le premier thermometre à alcool utilisé à Paris (pp. 230-231).

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Georgievics, G. v., und *Löwy, E.*, Ueber das Wesen des Färbeprocesses. Vertheilung von Methylenblau zwischen Wasser und mercerisirter Cellulose (pp. 345-351). *Kulisch, V.*, Zur Kenntniss der Condensationsvergänge zwischen o-Toluidin und a-Diketonen, sowie a-Ketonsäureestern (pp. 351-358). *Zulkowski, K.*, Zur Chemie des Corallins und Fuchsins (pp. 358-403).

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Schwarz, F., Ueber eine neue Polyphosphorsäure $H_5P_3O_{10}$ und einige Verbindungen derselben (pp. 249-267). *Jannasch* und *Schmitt*, Die quantitative Bestimmung des Fluors durch Austruben derselben als Fluorwasserstoffgas (pp. 267-274). *Jannasch* und *Schmitt*, Ueber Metalltrennungen in einen chlorwasserstoffstrome (pp. 274-283). *Thomsen, J.*, Ueber die mutmafsliche Gruppe inaktiver elemente (pp. 283-289). *Priwoznik, E.*, Ueber eine merkwürdige strukturveränderung des Glases durch Erwärmung (pp. 289-290). *Werner* und *Klein*, Ueber sogenannte amidochromsäure Salze (pp. 291-294). *Hofmann*,

K. A., und *Wilde, O. F.*, Weitere Mittheilungen ueber Nitrosoverbindungen des Eisens (pp. 295-303). *Wells, H. L.*, Ueber einige Jodverbindungen des Bleis mit ueberschüssigem Jod (pp. 304-311). *Lea, Carey*, Ueber die Beziehung der Farben von atom, ion und Molekul (pp. 312-328).

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Rudolphi, M., Ueber die Gültigkeit der Ostwaldschen Formel zur Berechnung der Affinitätskonstanten (pp. 385-426). *Ostwald, W.*, Ueber Physiko-Chemische Mess-Methoden (pp. 427-445). *Müller, E. W.*, Ueber dem dampfdruck des durch kupfervitriol und durch chlorbaryum gebundenen wassers (pp. 446-458). *Bredig, G.*, Ueber den Einfluss der Zentrifugalkraft auf chemiche systeme (pp. 459-472). *Pringsheim, N.*, Ueber chemiche Niederschläge in Gallerte (pp. 473-504). *Van T'Hoff* und *Goldschmidt, J. H.*, Das racemat von Wyronboff (pp. 505-512). *Moyan, J. L. R.*, Die Bestimmung von Cyanionen auf electrometrischen Wege (pp. 513-535). *Sutherland, W.*, Bemerkungen ueber die Thermodynamische Theorie der Kapillarität von Van der Waals (pp. 536-538). *Nasine, R.*, und *Carrara, G.*, Ueber das Brechungsvermögen des Sauerstoffs des Schwefels und des Stickstoffs in den Heterocyklischen Ringen (pp. 539-544). *Van Laar, J. J.*, Zur Berechnung von Losungswärmen aus der Loslichkeit (pp. 545-549). *Köppe, H.*, Bemerkungen zu Hedins Abhandlung. Ueber die Bestimmung isosmotisher konzentrationem durch zentrifugieren von Blut-mischungen (pp. 552-553). *Jahn, H.*, Berichtigung (pp. 550-551).

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Wróblewski, A., Zur Kenntniss des Pepsins (pp. 1-18). *Roos, E.*, Ueber die Einwirkung der Schilddrüse auf den Stoffwechel nehl vorversuchen ueber die art der wirksamen Substanz in derselben (pp. 19-41). *Wicke, A.*, und *Weiske, H.*, Ueber den einfluss einer Fett-resp. Stärkebeigabe auf die Ausnutzung der Nährstoffe sowie auf den Stickstoffumsatz und ansatz im Thierischen organismus (pp. 42-67). *Diechsel, E.*, Ueber die reduction alkalischer Kupferlösungen durch eiweisskörper (pp. 68-70). *Moraczewski, W. V.*, Ueber das Verhalten des Caseins in ammonialkalischer magnesium chloridlösung (pp. 71-78). *Stoklasa, J.*, Chemische Untersuchungen auf dem Gebiete der Phytospathologie (pp. 79-86)). *Sandmeyer, W.*, Ueber die ausmutzung des Paranuclëins im Thierischen organismus (pp. 87-89). *Tichomiroff, M.*, Ueber die Fällung von Toxalbuminen durch Nucleïnsäure (pp. 90-96). *Lieblein, W.*, Ueber die Bestimmung der Acidität des Harns (pp. 97-108). *Schulze, E.*, und *Frankfurt, S.*, Berichtigung (p. 108).

Anno xxv. Vol. 1. Fascicolo 6. *Gazetta Chimica Italiana.* (July, 1895.)

Longi, A., Sulla rapida determinazione della sostanza grassa nel latte e sopra un nuovo lacti buterrometro (pp. 441-451). *Andreocci, A.*, Sui quatro acidi santonosi (pp. 452-567).

APPENDIX I.

NOTICES OF BOOKS.

A Course of Elementary Practical Bacteriology. By A. A. Kauthack, M.D., M.R.C.P., Lecturer on Pathology and Bacteriology, and Curator of the Museum, St. Bartholomew's Hospital; and J. H. Drysdale, M.B., M.R.C.P., Casualty Physician, St. Bartholomew's Hospital; late Demonstrator of Bacteriology, University College, Liverpool. London: Macmillan & Co., 1895.

In these days of very numerous and often inefficient text-books, it is refreshing to glance through such a thoroughly practical guide as the little volume by Dr. Kauthack and Mr. Drysdale.

In the preface the authors state that they have divulged all their own "short cuts" and "secrets," some of which every laboratory possesses, and which, being regarded as almost sacred, are often withheld from the beginner, with the result that his operations end in failure. We can well believe this, for the details of procedure are most accurately described, and even a tyro, by following the directions, can scarcely go wrong. It is impossible for any one not engaged in teaching to appreciate the difficulty experienced by students in carrying out processes (very numerous in bacteriology) in which success depends almost entirely on care in following out minute details. The volume before us is divided into three parts, part i. dealing with general bacteriology and forming an elementary course arranged to extend over five or six weeks of three lessons a week.

Part ii. is devoted to bacteriological analysis, and includes the preparation of the various media, and the analysis of air, water, food, etc. Parts i. and ii. are especially arranged to suit the requirements of candidates for the Diploma in Public Health.

Part iii. comprises an introduction to bacteriological chemistry.

It is seldom we are able to recommend a book as heartily as in the present instance, and we look forward to seeing this little volume take its place as the best elementary guide to practical bacteriology for some years to come.

APPENDIX II.

CHEMICAL LITERATURE FOR AUGUST, 1895.

Vol. l. No. 296. *American Journal of Science.* (August, 1895.)

Gooch, F. A., and *Phelps, I. K.*, The Precipitation and Gravimetric Determination of Carbon Dioxide (pp. 101-104). *Kreider, D. A.*, Notes on Convenient Forms of Laboratory Apparatus (pp. 133-135).

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Eakle, S. A., and *Muthman, W.*, On the so-called Schneebergilé (pp. 244-246). *Sherzer, W. H.*, Native Sulphur in Michigan (pp. 246-249). *Wells, H. L.*, and *Boltwood, B. B.*, Double Salts of Cæsium Chloride with Chromium Trichloride and with Uranyl Chloride (pp. 249-252). *Gooch, F. A.*, and *Reynolds, W. G.*, The Reduction of the Acids of Selenium by Hydriodic Acid (pp. 254-258).

Vol. xvii. No 8. *Journal of the American Chemical Society.* (August, 1895.)

Osborne, S. B., The Chemical Nature of Diastase (pp. 587-604). *Hart, E.*, Note on the Purification of Glucenum Salts (pp. 604-605). *De Schweinitz, E. A.*, and *Dorsett, M.*, The Composition of the Tuberculosis and Glanders Bacilli (pp. 605-611). *Austen, P. T.*, and *Horton, W. A.*, A Convenient Form of Universal Hand-clamp (pp. 611-612). *Smith, E. F.*, and *Wallace, D. L.*, Electrolytic Separations (pp. 612-615). *Lenébarger, C. E.*, On the Vapour-Tensions of Mixtures of Volatile Liquids (pp. 615-652). *Smith, E. F.*, and *Harris, H. B.*, The Electrolytic Determination of Ruthenium (pp. 652-654). *Smith, E. F.*, and *Harris, H. B.*, The Action of Phosphorous Pentachloride upon the Dioxides of Zirconium and Thorium (pp. 654-656).

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Doremus, C. A., The Chemical History of a Case of Combined Antimonial and Arsenical Poisoning (pp. 667-682). *Smith, E. F.*, and *Hibbs, J. G.*, Action of Hydrochloric Acid Gas upon Salts of the Elements of Group V. of the Periodic System (pp. 682-686). *Smith, E. F.*, and *Matthews, J. M.*, Uranium Oxynitride and Uranium Dioxide (pp. 686-688). *Atkinson, E. A.*, and *Smith, E. F.*, The Separation of Iron from Beryllium (pp. 688-690). *Lenébarger, C. E.*, On the Vapour-Tensions of Mixtures of Volatile Liquids (pp. 690-718). *Hillebrand, W. F.*, Warning against the Use of Fluoriferous Hydrogen Peroxide in Estimating Titanium (pp. 718-719). *Wender, N.*, The Viscosimetric Examination of Butter for Foreign Fats (pp. 719-723). *Wesson, D.*, The Examination of Lard for Impurities (pp. 723-735). *Smith, E. F.*, and *Meyer, F. L.*, The Action of Haloid Acids in Gas Form upon the Salts of Elements of Group V. of the Periodic System (pp. 735-740). *De Koningh, L.*, Separation of Solid and Liquid Fatty Acids (pp. 740-741). *Leeds, A. R.*, Acetic Acids in Vinegar (pp. 741-744).

Vol. xx. No. 233. *The Analyst.* (August, 1895.)

Hughes, J., Feeding Experiments with Indian Peas, containing Lathyrus Sativus (pp. 169-174). *Clayton, E. G.*, Pseudo-Woollen Fabrics (p. 174). *Pearmain* and *Moor*, Note on Adulterated Sandal-Wood Oil (pp. 174-176). *Pearmain* and *Moor*, Note on a Sample of Adulterated Coffee (p. 176).

Vol. lxvii. No 393. *Journal of the Chemical Society.* (August, 1895.)

Ling, A. R., and *Baker, J. L.*, Action of Diastase on Starch. Preliminary Notice (pp. 702-709). *Brown, H. T.*, and *Morris, G. H.*, On the "Isomaltose" of C. J. Lintner (pp. 709-739). *Ling, A. R.*, and *Baker, J. L.*, Action of Diastase on Starch. Second Notice

(pp. 739-746). *Walker*, *J.*, and *Hambly*, *F. J.*, Transformation of Ammonium Cyanate into Urea (pp. 746-768). *Walker*, *J.*, and *Appleyard*, *J. R.*, Ethereal Salts of Ethanetetracarboxylic Acid (pp. 768-774). *Fenton*, *H. J. H.*, New Formation of Glycollic Aldehyde (pp. 774-780). *Chapman*, *A. C.*, Some Derivatives of Humulene (pp. 780-784). *Rennie*, *E. H.*, A Colouring Matter from *Lomatia ilicifolia* and *Lomatia longifolia* (pp. 784-794). *Cundall*, *J. T.*, Dissociation of Liquid Nitrogen Peroxide. II. The Influence of the Solvent (pp. 794-811). *Dyer*, *B.*, Kjeldahl's Method for the Determination of Nitrogen (pp. 811-817). *Perkin*, *A. G.*, and *Hummel*, *J. J.*, The Colouring and other Principles contained in Chay Root. II. (pp. 817-826). *Hirst*, *H. R.*, and *Cohen*, *J. B.*, A Modification of Zincke's Reaction (pp. 826-829). *Hirst*, *H. R.*, and *Cohen*, *J. B.*, A Method for Preparing the Formyl Derivatives of the Aromatic Amines (pp. 829-831). *Gladstone*, *J. H.*, and *Hibbert*, *W.*, The Molecular Refraction of Dissolved Salts and Acids (pp. 831-868). *Perman*, *E. P.*, Rate of Escape of certain Gases from Solutions of varying Concentration (pp. 868-888). *Rose*, *T. K.*, The Dissociation of Chloride of Gold (pp. 881-905). *Rose*, *T. K.*, Some Physical Properties of the Chlorides of Gold (pp. 905-906).

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Meldola, *R.*, and *Streatfield*, *F. W.*, Homonucleal Tri-derivatives of Naphthalene (pp. 907-914). *Walker*, *J. W.*, The Ethereal Salts of the Optically Active Lactic, Chloropropionic and Bromopropinic Acids (pp. 914-925). *Mills*, *C.*, Some New Azo-compounds (pp. 925-933). *Bedford*, *C. S.*, and *Perkin*, *A. G.*, Some Derivatives of Maclurin (pp. 933-937). *Perkin*, *A. G.*, and *Cope*, *F.*, The Constituents of Artocarpus integrifolia, I. (pp. 937-944). *Purdie*, *T.*, and *Bolan*, *H. W.*, Optically Active Methoxy- and Oropoxy-succinic Acids (pp. 944-957). *Purdie*, *T.*, and *Williamson*, *S.*, Ethereal Salts of Active Methoxy- and Ethoxy-succinic Acids (pp. 957-983). *Perman*, *E. P.*, Rate of Escape of certain Gases from Solutions of varying Concentration, Addendum (pp. 983-984). *Sonstadt*, *E.*, Note on the Reduction of Potassium Platinochloride (pp. 984-985).

Vol. xl. No. 244. *The Philosophical Magazine and Journal of Science.* (September, 1895.)

Liveing and *Dewar*, On the Refraction and Dispersion of Liquid Oxygen, and the Absorption Spectrum of Liquid Air (pp. 268-272). *Natanson*, *L.*, On the Critical Temperature of Hydrogen and the Theory of Adiabatic Expansion in the Neighbourhood of the Critical Point (pp. 272-282). *Runge*, *C.*, and *Paschen*, *F.*, On the Constituents of Clèveite Gas (pp. 297-303). *Dewar*, *J.*, and *Fleming*, *J. A.*, The Variation in the Electrical Resistance of Bismuth, when cooled to the Temperature of Solid Air (pp. 303-311).

Vol. lviii. No. 349. *Proceedings of the Royal Society.* (16th August, 1895.)

Ramsay, *W.*, Helium, a Gaseous Constituent of certain Minerals, I. (pp. 81-89). *Harcourt*, *A. V.*, and *Esson*, *W.*, On the Laws of Connection between the Conditions of Chemical Change and its Amount. III. Further Researches on the Reaction of Hydrogen Dioxide and Hydrogen Iodide (pp. 108-113). *Lockyer*, *J. N.*, On the new Gas obtained from Uraninite (pp. 113-116). *Moissan*, *H.*, Action du Fluor sur l'Argon (pp. 120-122). *Sloney*, *G. J.*, Note on the Motions of and within Molecules ; and on the Significance of the Ratio of the two Specific Heats in Gases (pp. 177-182). *Whetham*, *W. C. D.*, On the Velocities of the Ions (pp. 182-188).

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Tome vi. *Annales de Chimie et de Physique.* (September, 1895.)

Berthelot, Sur la Thermochimie des alcools (pp. 5-59). *Berthelot*, Sur la Thermochimie des aldéhydes et congénères (pp. 59-90). *Mallard* et *Le Chatelier*, Sur la variation qu'éprouvent, avec la température, les biréfringences du quartz, de la barytine et du disthène (pp. 90-115). *Welt, I.*, Contribution à l'étude des dérivés amyliques actifs (pp. 115-144).

Tome cxxi. No. 6. *Comptes Rendus hebdomadaires de l'Académie des Sciences.* (5th August, 1895.)

Guérin et *Macé*, Sur l'antitoxine diphtérique (p. 311). *Gourfein, D.*, Sur une substance toxique, extraite des capsules surrénales (pp. 311-314).

Tome cxxi. No. 7. (12th August, 1895.)

Le Chatelier, H., Sur quelques points de fusion et d'ébullition (pp. 323-326). *Astre, C.*, Sur certains dérivés potassiques de la quinone et de l'hydroquinone (pp. 326-328).

Tome cxxi. No. 8. (19th August, 1895.)

Schloesing, T., Sur les allumettes à pâtes explosives (pp. 334-336). *Varet, R.*, Recherches sur les combinaisons du cyanure de mercure avec les chlorures (pp. 348-351). *Lemoult, P.*, Recherches thermiques sur l'acide cyanurique (pp. 351-354). *Guinchant, J.*, Chaleur de combustion de quelques éthers β-cétoniques (pp. 354-357). *Bouffard, A.*, Détermination de la chaleur dégagée dans la fermentation alcoolique (pp. 357-360). *Nevière, G.*, et *Hubert A.*, Sur la gomme des vins (pp. 360-362). *Vaudin, L.*, Sur la migration du phosphate de chaux dans les plantes (pp. 362-364).

Tome cxxi. No. 9. (26th August, 1895.)

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Tome cxxi. No. 10. (2nd September, 1895.)

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Nalterer, K., Tiefsee-Forschungen im Marmara-Meer auf S. M. Schiff "Taurus" 1894 (pp. 405-580).

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Band ix. Heft 5 und 6. *Zeitschrift für Anorganische Chemie.* (18th August, 1895.)

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Anno xxv. Vol. 2. Fascicolo 1. *Gazzetta Chimica Italiana.* (18th Aug., 1895.)

Antony, U., e *Giglio, G.*, Sulla scomposizione idrolitica del cloruro ferrico (pp. 1-13). *Peratoner, A.*, e *Oddo, G.*, Sulla decomposizione di alcuni triazoturi (pp. 13-21). *Errera, G.*, Azione della idrossilamina sullo fialato di metile (pp. 21-25). *Errera, G.*, Azione della idrossilamina sull'anidride succinica (pp. 25-37). *Nasini, R.*, Osservazioni sopra l'arzo (pp. 37-46). *Anderlini, F.*, Sul dipropionato di diehlacehlenglicole ed osservazioni sui suoi omologhi superiori (pp. 46-58). *Zecchini, F.*, Contributo allo studio delle combinazioni orzaniche dell' ossigeno tetravalinte (pp. 58-63).

APPENDIX I.

NOTICES OF BOOKS.

A Manual of Physiology. With Practical Exercises. By G. N. Stewart, M.A., D.Sc., M.D. Edin., D.P.H. Camb., Professor of Physiology in the Western Reserve University, Cleveland. London: Baillière, Tindall & Cox, 1895.

Professor Stewart is to be congratulated on having written a book which will be useful alike to the beginner and to the advanced student.

Several of the Manuals of Physiology in use at the present time date from a period when the physiology taught in Medical schools was of a very meagre description, and as advances have been made and more thorough teaching has became necessary these manuals have been added to and amplified.

From this origin serious defects arise. The treatment of the subject lacks homogeneity, and except in those cases where whole chapters have been rewritten a sort of patchwork effect is produced, and a sense of dissatisfaction arises which often leads to the student neglecting his reading altogether.

Such defects as these are altogether avoided in those rare cases where an enthusiastic worker who possesses at once a power of clear description and lucid explanation and a thorough grasp of the subject in its latest developments can be persuaded to make use of his knowledge to smooth the way for beginners in his chosen science.

We have in the book before us an example of such a case. The author, besides being one of the most prominent of the younger generation of physiologists, has had a very considerable experience in training medical students and others in the paths of physiology, and of this experience he has taken full advantage in writing his book. Briefly, the plan adopted is that of providing at the end of each section of theoretical matter some pages of directions for practical exercises which shall illustrate the phenomena described. In this way a class of students may be taken through the theoretical and practical work together, to the great advantage of everybody concerned; it often being the case at present either that the students do not understand the theoretical bearings of the practical work they are called upon to do, or that they fail to grasp the full significance of the book work because it is not fully illustrated by laboratory exercises. This must often be the case when the courses are conducted by two different teachers. Where the same man is teacher and lecturer things are at their best, and students have the best possible chance of learning their subject in a satisfactory manner, and where this is impossible the next best thing is for the theoretical and practical work to be selected by the same hand, and set forth in a manner to mutually illustrate each other.

Dr. Stewart has gone very thoroughly over the ground, and has not scrupled where necessary to introduce histological descriptions, *e.g.*, in describing absorption of fat by the epithelium of the villi, and the changes undergone by the cells of the salivary glands during activity.

On the other hand, general histology is excluded, and the 786 pages are devoted to legitimate physiology throughout.

The volume is brought well up to date, and gives evidence of having been written by one actively working at the subject. The plan of using two different types has been adopted, and will prove useful in defining portions which may with advantage be omitted in reading through the work a first time.

Defects there are and misprints, and some of the figures will appear rough to fastidious eyes, but many of us will recognise in these rough drawings a strong likeness to familiar pieces of apparatus with which, in the past, work which will live has been done.

The print and paper are excellent, and the coloured figures well selected and admirably reproduced.

A Text-book of the Science and Art of Breadmaking. By William Jago, F.I.C., F.C.S. London: Simpkin, Marshall, Hamilton, Kent & Co., Ltd., 1895.

In *The Science and Art of Breadmaking* Mr. Jago has given to the world a volume containing an immense amount of information on almost all subjects connected with bread-making.

Recognising that many of his readers would probably have but scanty scientific libraries to refer to, the author has prefaced the more serious parts of the work with some pages of elementary physical definitions and notes on the properties of the more important chemical elements and compounds. The construction and use of the microscope are shortly described, and then follow chapters in which the carbohydrates and proteids are adequately treated. The important subjects of Enzymes and Fermentation come in for a good share of attention and are treated in such a way as to be intelligible to any one who is likely to refer to the book. The plates in this section are numerous and useful.

In the chapters on breadmaking, bakehouse design, and flour testing, immense stores of information are laid bare, whilst the section on the cause of and cures for sour bread will be f service to many.

Altogether we expect the book will take its place as one of the most important works of reference in the hands of those connected with the baking trade.

Leitfaden für histiologische untersuchungen. Von Dr. B. Rawitz. Zweite umgearbeitete und vermehrte auflage. Jena: Gustav Fischer, 1895.

This convenient little volume forms an excellent manual of histological technology. A very large number of fixing, hardening, and staining methods are described, and a useful chapter on microtomes and microtome knives is also included. The first part of the volume ends with a brief account of the process of reconstructing in wax the form and structural details observed in a series of sections. The second part deals with the practical application of the methods previously described, to the various tissues of different animals. The book is decidedly worthy of commendation, and the author's own notes and criticisms on the many processes described will be valuable to histologists.

The October number of the *Photogram* (London: Dawbarn & Ward) contains a useful note on Photographing Coins and Medals; good reproductions of Photographs of Maori types by A. Iles, of Thames, N.Z.; a continuation of Mr. Charles Gamble's papers on Gums, Resins, Balsams, Oils, and Waxes; and a specially welcome paragraph on the Photography of the Retina, besides other articles and notes.

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An Introduction to the Study of Rocks. British Museum (Natural History): Printed by order of the Trustees, 1895.

The International Journal of Microscopy and Natural Science. October, 1895. Edited by Alfred Allen. London: Baillière, Tindall & Cox; Simpkin, Marshall, Hamilton, Kent & Co.

Die Leitungsbahnen in Gehirn und Rückenmark. W. von Bechterew. Leipzig: Eduard Besold, 1894.

Analytical Key to the Natural Orders of Flowering Plants. Franz Thonner. London: Swan Sonnenschein & Co., 1895.

The Splash of a Drop. Prof. A. M. Worthington, M.A., F.R.S. London: Society for Promoting Christian Knowledge, 1895.

APPENDIX II.

CHEMICAL LITERATURE FOR SEPTEMBER, 1895.

Vol. l. No. 298. *American Journal of Science.* (October, 1895.)

Kreider, D. A., Quantitative Determination of Perchlorates (pp. 287-298). *Packard, R. L.*, Occurrence of Copper in Western Idaho (pp. 298-301). *Crookes*, W., Spectrum of Helium (pp. 302-309).

Vol. xx. No. 234. *The Analyst.* (September, 1895.)

Richmond, H. D., Duclaux's Method for the Estimation of "Volatile Fatty Acids," the Laws governing "Volatility" deduced therefrom, and their application to Analysis, more especially of Butter (pp. 193-198).

Vol. xlvii. No 395. *Journal of the Chemical Society.* (October, 1895.)

Pope, W., Orthobenzoicsulphinide (pp. 985-990). *Perkin, A. G.*, Derivatives of β-Resorcylic acid (pp. 990-999). *Glendinning, T. A.*, Note on the Gravimetric Estimation of Maltose by Fehling's Solution (pp. 999-1002). *Ruhemann, S.*, and *Orton, K. J. P.*, Studies in the Malonic Acid Series (pp. 1002-1013).

Vol. xl. No. 245. *The Philosophical Magazine and Journal of Science.* (October, 1895.)

Hibbert, W., The Gladstone "Law" in Physical Optics, and the True Volume of Liquid Matter (pp. 321-345). *Stoney, G. J.*, Of the Kinetic Theory of Gas, regarded as illustrating Nature (pp. 362-383). *Jones, H. C.*, On the Cryoscopic Relations of Dilute Solutions of Cane Sugar and Ethyl Alcohol (pp. 383-393).

Tome vi. *Annales de Chimie et de Physique.* (October, 1895.)

Berthelot, Sur la Thermochimie des acides organiques (pp. 145-232). *Berthelot*, Recherches sur la Thermochimie des composés organiques azotés (pp. 232-296). *Moissan, H.*, Etude du bore amorphe (pp. 296-320).

Tome xiii. Nos. 16-17. *Bulletin de la Société Chimique de Paris.* (5th September, 1895.)

Sabatier, P., et *Senderens, J. B.*, Sur la réduction de l'oxyde azotique par le fer ou le zinc humides (pp. 790-794). *Sabatier, P.*, et *Senderens, J. B.*, Sur la réduction de l'oxyde azoteux par les métaux en présence de l'eau (pp. 794-798). *Moissan, H.*, Sur la vaporisation du carbone (pp. 798-803). *Moissan, H.*, Réduction de l'alumine par le charbon (pp. 803-806). *Moissan, H.*, Etude des différentes variétés de graphite (pp. 806-811). *Moissan, H.*, Déplacement du carbone par le bore et le silicium dans la fonte en fusion (pp. 811-814). *Moissan, H.*, Etude des graphites du fer (pp. 814-818). *Moissan, H.*, Préparation, au four électrique, de graphites foissonants (pp. 818-820). *Arth, G.*, Sur le calcul du pouvoir calorifique des houilles d'après la règle du Dulong (pp. 820-823). *Freundler, P.*, Sur les variations du pouvoir rotatoire dans la série β-méthyladipique (pp. 823-834). *Béhal, A.*, Dérivés campholéniques. Campholénonitriles et campholénamides (pp. 834-841). *Béhal, A.*, Acides campholéniques et campholènes. II. (pp. 841-847). *Mamert, R. T.*, Sur la non-existence de la stéréoisomérie dans les dérivés aminobuténédioïques (pp. 847-853). *Mamert, R. T.*, Sur les aminobutèneamidoates d'éthyle (pp. 853-857). *Delacre, M.*, Sur la triphényléthanone et la triphényléthanolone (pp. 857-862). *Fonzes-Diacon*, Nouvelle préparation du glycerose (pp. 862-863).

Tomes xiii.-xiv. Nos. 18-19. (5th October, 1895.)

Massol, G., Considérations générales sur les points de fusion des acides de la série grasse (pp. 865-870). *Sabatier, P.*, et *Senderens, J. B.*, Action de l'oxyde azoteux sur les métaux et sur les oxydes métalliques (pp. 870-873). *Granger, A.*, Action des combinaisons halogénées du phosphore sur le cuivre métallique (pp. 873-874). *Villiers, A.*, et *Fayolle*, Sur la recherche de l'acide borique. Vins boriqués (pp. 874-877). *Jay, H.* et *Dupasquier*, Sur le dosage de l'acide borique (pp. 877-880). *Lescoeur, H.*, e *Lemaire, Cl.*, Sur le dosage volumétrique du zinc (pp. 880-882). *Fournier*, Sur la préparation de carbures diéthyléniques (pp. 882-885). *Cavalier, J.*, Sur les éthers phosphoriques de l'alcool allylique (propénol) : Acide allylphosphorique (pp. 885-888). *Bayrac, H. P.*, Préparation du paraétheyltoluéne. Sur deux sels monosulfoconjugués barytiques du paraéthyltoluène. Sur deux méthylparaéthylphénols (pp. 889-894). *Bayrac, H. P.*, Préparation du paracymène normal (parapropyltoluène). Sur un nouveau phénol : le méthylparapropylphénol α (pp. 894-897). *Bayrac, H. P.*, Sur l'indophénol du méthylparaéthylphénol α et sur une nouvelle quinone : la paraéthyltoluquinone (pp. 897-898). *Grimaux, E.*, Action du chlorure de zinc sur la résorcine (pp. 899-901). *Burcker, E.*, Action de l'anhydride camphorique sur le benzène en présence du chlorure d'aluminium. Formation d 'un nouvel acide cétonique (pp. 901-905). *Prud'homme, M.*, Sur les matières colorantes sulfonées dérivées du triphénylméthane. Étude des " bleus patentés " (pp. 905-913). *Prud'homme, M.*, Réduction des nitrotétraméthyldiamidotriphénylméthanes (pp. 913-915). *Ackermann, E.*, La molécule de la quinine renferme un groupement développateur (pp. 915-917). *Oliviéro*, Étude sur l'huile essentielle de valériane (*Valériana officinialis*), variété sauvage (pp. 917-927). *Hébert, A.*, Note sur la sère (pp. 927-932).

Tome cxxi. No. 11. *Comptes Rendus hebdomadaires de l'Académie des Sciences.* (9th September, 1895.)

Matignon, C., et *Deligny*, Sur les substitutions nitrées (pp. 422-424). *Maquenne, L.*, Sur l'explosion des gaz endothermiques (pp. 424-427).

Tome cxxi. No. 12. (16th September, 1895.)

Malbot, H. et *A.*, Recherches sur les phosphates d'Algérie. Cas d'une roche phosphatée de Bougie, présentant la composition d'un superphosphate (pp. 442-445).

Tome cxxi. No. 13. (23rd September, 1895.)

Moissan, H., Sur un échantillon de carbon noir du Brésil (pp. 449-450). *Griffiths, A. B.*, et *Platt, C.*, Sur la composition de la pélagéine (pp. 451-452).

Tome cxxi. No. 14. (30th September, 1895.)

Faure, C., Sur un nouvel engrais azoté : le cyanate de calcium (p. 463). *Klobb, T.*, Synthèses au moyen de l'éther cyanacétique (pp. 463-465). *Béhal, A.*, Constitution des acides produits dans l'oxydation de l'acide campholénique inactif (pp. 465-468).

15e Année. Tome ii. No. 6. *Journal de Pharmacie et de Chimie.* (15th September, 1895.)

Villiers, A., et *Fayolle*, Sur la recherche de l'acide borique (pp. 241-244). *Dupasquier, J.*, Sur le dosage de l' acide borique (pp. 244-247). *Roeser*, Note sur les soudures des boîtes de conserves alimentaires (pp. 247-250). *Roeser*, Note sur l'influence de la lumière sur un perchlorure de fer liquide (pp. 250-252). *Allain*, Sur un moyen simple d'obvier à la décompostion du chloroforme à l'aide du soufre (pp. 252-255).

15e Année. Tome ii. No. 7. (1st October, 1895.)

Denigès, G., Trois nouveaux réactifs des azotites (pp. 289-293). *Vaudin, L.*, Sur l'emploi du chromate de plomb pour colorer les pâtisseries communes (pp. 293-294). *André, C.*, Un nouvel anesthésique local : le gaïàcol (pp. 294-298).

Band ccxxxiii. Heft 6. *Archiv der Pharmacie.* (10th September, 1895.)

Partheil, A., Ueber die Bestimmung des Glycerins im Weine, etc. (pp. 391-398). *Winterstein, E.*, Chemische Zusammensetzung von Pachyma Cocos und Mylitta lapidescens (pp. 398-410). *Beckurts, H.*, Zur Kenntnis der Angostura-Alcaloide (pp. 410-423). *Beckurts, H.*, und *Seiler, H.*, Ueber Fettuntersuchungen mit dem Refractometer (pp. 423-429). *Beckurts, H.*, und *Oelze, F.*, Zur Kenntnis des Hirschtalgs (pp. 429-430). *Plugge, P. C.*, Ueber das Vorkommen von Cytisin in verschiedenen Papilionaceen (pp. 430-441). *Plugge, P. C.*, Ueber das Matrin, das Alcaloid der Sophora augustifolia (pp. 441-443). *Hallström, K. T.*, Anatomische Studien ueber den Samen der Myristicaceen und ihre Arillen (pp. 443-470).

Band ccxxxiii. Heft 7. (25th September, 1895.)

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Jahrgang xxviii. No. 14. *Berichte der Deutschen Chemischen Gesellschaft.* (16th September, 1895.)

Friedlander, P., und *Weisberger, J.*, Ueber einige parasubstituirte α-Naphtoësäuren (pp. 1838-1843). *Von Norwall, K. F.*, Erwiderung (pp. 1843-1845). *Pechmann, H. v.*, und *Wedekind, E.*, Ueber das Aldol des Diacetyls (pp. 1845-1847). *Pechmann, H. v.*, Ueber das Verhalten fetter Diazoverbindungen gegen schwefligsaure Alkalien (pp. 1847-1850). *Konowalow, M.*, Ueber eine empfindliche Reaction der primären und secundären Nitroverbindungen (pp. 1850-1852). *Konowalow, M.*, Nitrirende Wirkung der Salpetersäure auf den Charakter gesättigter Verbindungen besitzende Kohlenwasserstoffe und deren Derivate (pp. 1852-1866). *Erlenmeyer, E., Jun.*, Ueber Isodiphenyloxäthylamin, ein Condensations product von Benzaldehyd und Glycocoll (pp. 1866-1870). *Gassmann, C.*, und *Krafft, E.*, Ueber Eugenolderivate (pp. 1870-1872). *Knoll, R. J.*, und *Cohn, P.*, Ueber Naphtylindoxazen (pp. 1872-1874). *Syniewski, V.*, Ueber Methylcarbonate einiger mehrwerthigen Phenole (pp. 1874-1876). *Jakimowicz, P.*, Ueber die Darstellung von Thymolphtalid (pp. 1876-1877). *Pinner, A.*, Ueber die Addition von Brom zu Tetrolsäure (pp. 1877-1887). *Bamberger, E.*, und *Meimberg, F.*, Einige weitere Beobachtungen ueber Azofarbstoffe (pp. 1887-1897). *Söderbaum, H. G.*, Ueber einige von Diphenoloxäthylamin sich ableitende heterocyclische Basen (pp. 1897-1904). *Pictet, A.*, und *Crépieux, P.*, Ueber Phenyl und Pyridyl-pyrrole und die Constitution des Nicotins (pp. 1904-1913). *Brühl, J. W.*, Ueber Esterificerung und Verseifung (pp. 1913-1915). *Oddo, G.*, Ueber die constitution der Isonitrosoketone (pp. 1915-1917). *Salkowski, H.*, Ueber Esterbildung bei aromatischen Amidosäuren (pp. 1917-1923). *Le Bel, J. A.*, Zusatz zu H. P. Walden's Veröffentlichung ueber active Halogenverbindungen (pp. 1923-1925). *Widman, O.*, Ueber Phenyl und Tribromphenylazocarbonsäure (pp. 1925-1932). *Pinner, A.*, Ueber Nicotin (pp. 1932-1935). *Fromm, E.*, Ueber geschwefelte Abkömmlinge der Imidodicarbonsäure (pp. 1935-1940). *Cross, C. F., Bevan, E. J.*, und *Smith, C.*, Ueber die Frage nach dem Ursprung ungesättigter Verbindungen in der Pflanze (pp. 1940-1946). *Friedlander, P.*, Untersuchungen ueber isomere Naphtalinderivate (pp. 1946-1954). *Bruns, E.*, Ueber Darstellung der drei Nitraniline (pp. 1954-1955). *Wallach, O.*, Ueber Isomerien innerhalb der Terpenreihe (pp. 1955-1968). *Kuhling, O.*, Ueber die Oxydation des Tolualloxazins II. (pp. 1968-1971). *Lippmann, Ed.*, Ueber das Apochinin und sein Drehungsvermögen (pp. 1971-1973). *Fischer, E.*, Ueber den Volemit, einen neuen Heptit (pp. 1973-1975). *Fischer, E.*, und *Fay, I. W.*, Ueber Idonsäure, Idose, Idit und Idozuckersäure (pp. 1975-1983). *Wislicenus, H.*, und *Kauffmann, L.*, Notiz ueber die Reactionen des amalgamirten Aluminiums (pp. 1983-1986). *Koenigs, W.*, Weitere Beiträge zur Kenntniss des Merochinens (pp. 1986-1991). *Ladenburg, A.*, Ueber Racemie und ueber Wärmestörungen beim Vermischen von Flüssigkeiten (pp. 1991-1995).

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H.*, Ueber das Monoäthyltrimethylbenzol (pp. 2027-2029). *Ilosvay*, *L.*, Neue Reagentien des Wasserstoffsuperoxyds (pp. 2029-2031). *Ilosvay*, *L.*, Ueber das gegenseitige Verhalten des Wasserstoffsuperoxyds und der salpetrigen Säure in sehr verdünnten Lösungen (pp. 2031-2033). *Salzer T.*, Ueber das Krystallwasser (pp. 2033-2036). *Künne*, *H.*, Zur Kenntnis der Amidoketone der Fettreihe (pp. 2036-2044). *Klages*, *A.*, und *Knoevenagel*, *E.*, Ueber Dihydro-*p*-Chlorxylol (pp. 2044-2048). *Knoevenagel*, *E.*, Zur Darstellung trockner Diazosalze (p. 2048). *Freidheim*, *C.*, und *Euler*, *H.*, Zur quantitativen Bestimmung des Molybdäns (pp. 2061-2067). *Freidheim*, *C.*, Maassanalytisehe Bestimmung von Molybdäntrioxyd und Vanadinpentoxyd nebeneinander (pp. 2067-2073). *Hantzsch*, *A.*, und *Schultze*, *O. 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W.*, Ueber das natürlieh vorkommende Methylheptenon, Linalool und Geraniol (pp. 2126-2137). *Tiemann*, *F.*, und *Schmidt*, *R.*, Ueber die Umwandlung von d- und l-Linalool und Geraniol in Terpinhydrat (pp. 2137-2141). *Tiemann*, *F.*, und *Semmler*, *F. W.*, Ueber den Abbau des Dihydroearveols und Limonens (pp. 2141-2151). *Mahla*, *F.*, und *Tiemann*, *F.*, Zum Abbau der Camphersäure (pp. 2151-2166). *Tiemann*, *F.*, Zur Kenntniss der Campholengruppe (pp. 2166-2189). *Semmler*, *F. W.*, Ueber ein optisch Aetives Terpineol (pp. 2189-2191). *Tiemann*, *F.*, Ueber das Bromphenylhydrazon und Semiearbazon des d-Camphers (pp. 2191-2192). *Michaelis*, *A.*, und *Erdmann*, *G.*, Ueber die Thionylamine der Amidoazoverbindungen und der Naphtylendiamine (pp. 2192-2205). *Michaelis*, *A.*, und *Luxembourg*, *K.*, Ueber n-Phosphine und n-Phosphoniumverbindungen (pp. 2205-2212). *Michaelis*, *A.*, und *Kuhlmann*, *F.*, Ueber Tolyl- und Phenylanil-phosphoniumverbindungen (pp. 2212-2217). *Töhl*, *A.*, Spaltung des Thiophens dureh Piperidin (pp. 2217-2220). *Stoermer*, *R.*, und *Dzimski*, *O.*, Ueber einige disubstituirte Amidoaeetone (pp. 2220-2227). *Krümmel*, *H.*, Ueber die Einwirkung von Halogenen und Thiophosgen auf Amidoxime (pp. 2227-2234). *Will*, *W.*, Naphtazarin aus δ-Tetranitronaphtalin (1.2.5.8.) 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Kostanecki, St. v., Ueber ein nener Reductionsproduct des Xanthons (pp. 2310-2312). *Bourgeois, E.*, Ueber die Einwirkung der Monobromderivate der aromatischen Kohlenwasserstoffe auf die Bleimercaptide (pp. 2312-2331).

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Herzig, J., und *Meyer, H.*, Weitere Bestimmungen des Alkyls am Stickstoff (pp. 599-609). *Schrötter, H.*, Beiträge zur Kenntniss der Albumosen. II. (pp. 609-619). *Gregor, G.*, Zur Constitution des Resacetophenons (pp. 619-630). *Lippmann, E.*, und *Fleissner, F.*, Ueber die Hydrirung des Chinins (pp. 630-639). *Nalterer, K.*, Ueber einige von dem Botaniker Dr. Otto Stapf aus Persien mitgebrachte salzhaltige Erd- und Wasserproben und deren Beziehungen zu Meeresablagerungen. Mit einem Anhang, enthaltend die Analyse einer Wasserprobe aus dem Gaukhane-See Ausgeführt von Dr. Adolf Heider (pp. 639-674). *Vortmann, G.*, Elektrolytischc Bestimmung der Halogene. II. (pp. 674-684). *Jolles, A.*, Ueber eine einfache und empfindliche Methode zum qualitativen und quantitativen Nachweis von Quecksilber im Harn (pp. 684-693). *Blumenfeld, S.*, Ueber Cinchomeronsäurederivate (pp. 693-720).

Band x. Heft 1, 2. *Zeitschrift für Anorganische Chemie.* (18th Sept., 1895.)

Richards, T. W., und *Rogers, E. F.*, Neubestimmung des Atomgewichtes von Zink (pp. 1-25). *Arctowski, H.*, Notiz ueber die Krystallisation des Broms (pp. 25-27). *Arctowsik, H.*, Zur Krystallographie des Quecksilberchlorids (pp. 27-31). *Kruss, G.* und *H.*, Eine neue Methode der quantitativen spektralanalyse (pp. 31-44). *Lorenz, R.*, Ueber die Darstellung von Zinntetrachlorid in grossen Quantitäten (pp. 44-47). *Vandenberghe, A.*, Ucber Molybdandihydroxychlorid (pp. 47-60). *Neubauer, H.*, Ueber Phosphorsäurebestimmung nach der Molybdän-methode (pp. 60-66). *Marchetti, G.*, Studium einiger Fluoride und Oxyfluoride des Kaliums (pp. 66-74). *Lorenz, R.*, Ueber die Umwandlung von Chlor in Salzsäure (pp. 74-78). *Lorenz, R.*, Versuche zur Begründung eines gemeinsamen elektrolytischen Zink und Bleigewinnungsverfahrens (pp. 78-117). *Clever, A.*, und *Muthmann, W.*. Zur Kenntnis der Verbindungen des Selens mit dem Arsen (pp. 117-148). *Schulze, J.*, Ueber einige Salze der Chrom- und Bichrom-säure mit den Schwermetallen (pp. 148-155). *Thomsen, J.*, Ueber die Farbe der Ionen als Funktion der Atomgewichte (p. 155).

Anno xxv. Vol. 2. Fascicolo 2. *Gazzetta Chimica Italiana.* (18th Sept., 1895.)

Schiff, R., e *Prosio, P.*, Sulla sintesi di derivati della piridina per l'azione dell 'etere acetacetico sulle aldeidi in presenza di ammoniaca (pp. 65-91). *Ampola, G.*, e *Manuelli, C.*, Il bromoformio in crioscopia (pp. 91-101). *Oddo, G.*, e *Manzella, E.*, Ricerche su alcuni cementi italiani ed esteri (pp. 101-113). *Oddo, G.*, e *Manzella, E.*, Ricerche sui fenomeni che avvengono durante la presa del cementi (pp. 113-127). *Anderlini, F.*, Sopra alcune questioni relative alla rifrazione atomica del' issigeno (pp. 127-162). *Angeli, A.*, e *Rimini, E.*, Sopra alcuni bromoderivati della serie della canfira (pp. 162-164).

APPENDIX I.

NOTICES OF BOOKS.

The Flowering Plants of Western India. By Rev. Alexander Kyd Nairne. London: W. H. Allen & Co.

Although Mr. Nairne has brought together a large number of interesting facts concerning the plants of Western India, there is evidence on almost every page of his little book of his incompetence for the task which he has attempted. Indeed the botany is beyond serious criticism, and the extracts from a great variety of books relating to the properties, uses, and distribution of the plants were evidently not made by a person sufficiently acquainted with the subject to exercise any sort of discrimination.

An Introduction to Chemical Crystallography. By Andreas Fock. Translated and edited by William J. Pope. Oxford: The Clarendon Press, 1895.

This small book of 184 pages is intended to supply the student with such of the important truths of crystallography as are of especial service in the study of chemical problems. Of course it is impossible within the limits of so small a volume to deal exhaustively with the various divisions of the subject, and occasionally the treatment is far from adequate. This is particularly evident in the chapters dealing with the nature of crystals and the laws of crystallisation, and again in the chapter on solution, which is the most superficial in the book. On the other hand the book contains much that is excellent. It is significant that the work of Willard Gibbs finds a place—work of such importance to the chemist and so long overlooked by him, no doubt on account of its generality. Roozeboom's development of the analogy between the solubility of mixed crystals and the evaporation of mixed liquids is also treated with sufficient fulness to give the reader an idea of its scope and importance. The chapters on double salts, isomorphism, physical isomerism, and the relations between crystalline form and chemical composition are particularly good, and contain much having the most direct bearing on chemistry.

On discovering a new substance nothing delights the heart of the chemist more than to obtain it in well-defined crystals, yet when he obtains his heart's desire after analysing the substance he, in general, determines its melting-point, and considers that the determination of other physical properties is unnecessary. The shortsightedness of this policy is clearly brought home by numerous instances of substances having several melting-points varying with their crystalline condition, and the suggestion made in the translator's preface that observations in the polarising microscope should always be made with new crystalline substances merits the attention of all workers in this field of chemical research.

The book would be improved by the addition of a chapter dealing with the crystallographic terms used—axial ratios, mass-points, etc.—many of which are not familiar to the chemist. Nevertheless the book may, on the whole, be recommended as giving a brief and interesting historical account of the nature, origin, preparation, and properties of crystals.

APPENDIX II.

CHEMICAL LITERATURE FOR OCTOBER, 1895.

Vol. l. No. 299. *American Journal of Science.* (November, 1895.)

Palmer, A. D. F., Wave Length of the D_3 Helium Line (pp. 357-359). *Hill, E. A.*, Additional Notes on Argon and Helium (pp. 359-377). *Wells, H. L.*, and *Hurlburt, F. B.*, Ammonium-Cuprous Double Halogen Salts (pp. 390-394). *Gooch, F. A.*, and *Evans, P. S.*, Reduction of Selenic Acid by Hydrochloric Acid (pp. 400-402). *Gooch, F. A.*, and *Scoville, W. S.*, Reduction of Selenic Acid by Potassium Bromide in Acid Solution (pp. 402-405).

Vol. xvii. No. 10. *Journal of the American Chemical Society.* (October, 1895.)

Blair, A. A., and *Whitfield, J. E.*, Ammonium Phosphomolybdate and the Reducing Action of Zinc in the Reductor (pp. 747-760). *Rosell, C. A. O.*, The Ferrates (pp. 760-769). *Beeson, J. L.*, A Simple and Convenient Apparatus for Estimating the Water-holding power of Soils (pp. 769-771). *Gill, A. H.*, A Modification of Himman's Explosion Pipette (pp. 771-772). *Lunge, W.*, Estimation of Sulphur in Pyrites (pp. 772-775). *Prescott, A. B.*, The Periodides (pp. 775-781). *Dunnington, F. P.*, An Improved Gas Regulator (pp. 781-783). *Munroe, C. E.*, Inspection of Cotton for Use in the Manufacture of Gun-cotton (pp. 783-789). *Fleck, H.*, and *Bassett, L. L.*, Reduction with Magnesium Amalgam (pp. 789-791). *De Benneville, J. S.*, Two Definite Carbides of Iron with Chromium (Molybdenum and Tungsten) (pp. 791-801). *Phillips, F. C.*, On the Possibility of the Occurrence of Hydrogen and Methane in the Atmosphere (pp. 801-809). *Harris, E. O.*, A New Generator (pp. 809-811). *Krug, W. H.*, The Determination of Tannin by Metallic Oxides (pp. 811-814). *Heath, G. L.*, The Estimation of Sulphur in Refined Copper (pp. 814-822). *Kebler, L. F.*, Acidimetric Estimation of Vegetable Alkaloids (pp. 822-832).

Vol. xvii. No. 11. (November, 1895.)

Williams, R. P., An Electrolytic Process for the Manufacture of White Lead (pp. 835-842). *Venable, F. P.*, The Chlorides of Zirconium (pp. 842-843). *Noyes, W. A.*, *McTaggert, J. R.*, and *Craver, H. W.*, The determination of the Heating Effects of Coal (pp. 843-849). *Whitehead, C.*, Tellurium ; Its separation from Copper Residues, with Notes on some New Reactions (pp. 849-855). *Noyes, W. A.*, and *Dorrance, J. T.*, The Electrolytic Reduction of paranitro Compounds in Sulphuric Acid Solution (pp. 855-859). *Prescott, A. B.*, and *Trowbridge, O. F.*, Periodides of Pyridine (pp. 859-869). *Andrews, L.*, On the Analysis of Alloys of Lead, Tin, Antimony, and Arsenic (pp. 869-873). *Shimer, P. W.*, The Determination of Graphite in Pig Iron (pp. 873-877). *Griffith, A. B.*, and *Platt, C.*, On the Composition of Pelagine (pp. 877-878). *Wells, H. L.*, and *Mitchell, W. L.*, On the Volumetric Determination of Titanic Acid and Iron in Ores (pp. 878-883). *Barton, G. E.*, Arsenic in Glycerol (pp. 883-890). *Noyes, W. A.*, and *Watkins, W. H.*, The Occurrence of Trimethylene Glycol as a By-Product in the Glycerol Manufacture (pp. 890-891). *Phillips, F. C.*, The Evolution Method for the Determination of Sulphur in White Cast Iron (pp. 891-901). *Cushman, A. S.*, and *Hayes-Campbell, J.*, On the Volumetric Determination of Lead (pp. 901-904). *Baskerville, C.*, Reaction between Copper and Concentrated Sulphuric Acid (pp. 904-912).

Vol. xvii. No. 8. *American Chemical Journal.* (October, 1895.)

White, J., On Sulphonphthaleins and some Related Compounds (pp. 545-556). *Jones, W.*, Sulphonphthalein derived from Orthosulphoparatoluic Acid (pp. 556-570). *Miller, A. S.*, A Study of the Chemical Behaviour of Ammonia toward Ferric Chloride and Ferrous Chloride (pp. 570-579). *Jackson, C. L.*, and *Grindley, H. S.*, Action of Sodic Alcoholate on Chloranil. Acetal Derived from Substituted Quinines (pp. 579-607). *Mead, J. L.*, and *Kremers, E.*, From Pinene to Carvacrol (pp. 607-612). *Wheeler, H. L.*, On Halogen Addition Products of the Anilides (pp. 612-620). *Noyes, W. A.*, and *Ellis, R. M.*, Synthesis of Diphenylbiphenyl and its Identification as Benzerythrene (pp. 620-622).

Vol. xx. No. 235. *The Analyst.* (October, 1895.)

Richmond, H. D., Duclaux's Method for the Estimation of "Volatile Fatty Acids," the Laws Governing "Volatility" deduced therefrom, their Application to Analysis, more Especially of Butter (concluded) (pp. 217-220).

Vol. lxvii. No. 396. *Journal of the Chemical Society.* (November, 1895.)

Chikashigé, M., Mercury Perchlorates (pp. 1013-1018). *Luxmoore, C. M.*, α-Ethylene-Dihydroxylamine Dihydrobromide (pp. 1018-1019). *Luxmoore, C. M.*, Alleged Isomerism of Potassium Nitrososulphate (pp. 1019-1024). *Heycock, C. T.*, and *Neville, F. H.*, On the Freezing Points of Gold and Silver (pp. 1024-1030).

Vol. xiv. No. 10. *Journal of the Society of Chemical Industry.* (31st Oct., 1895.)

Briggs, T. L., Note on the Manufacture of Phenylamidoazobenzene and some Derivatives (pp. 851-852). *Schweitzer, H.*, Adulteration and Substitution of Drugs (pp. 852-853).

Vol. xl. No. 246. *Philosophical Magazine and Journal of Science.* (November, 1895.)

Lehfeldt, R. A., On the Properties of a Mixture of Liquids (pp. 397-413). *Waterman, F. A.*, An Improved Calorimeter for the Application of the Method of Mixtures (pp. 413-421). *Sutherland, W.*, The Viscosity of Mixed Gases (pp. 421-431). *Estreicher, T.*, On the Pressures of Saturation of Oxygen (pp. 454-463). *Wilde, H.*, On Helium and its place in the Natural Classification of Elementary Substances (pp. 466-472). *Pickering, S. U.*, Self-recorded breaks in the Properties of Solutions (pp. 472-476).

Series 7. Tome v. *Annales de Chimie et de Physique.* (November, 1895.)

Maneuvrier, G., Nouvelle méthode de détermination du rapport $\frac{C}{c}$ pour l'air et d'autres gaz (pp. 321-377). *Denigès, G.*, Sur une nouvelle méthode cyanimétrique et sur les applications qui en dérivent (pp. 377-428). *Moissan, H.*, Sur le dosage du bore (pp. 428-432).

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APPENDIX I.

NOTICES OF BOOKS.

Die Leitungsbahnen in Gehirn und Rückenmark. By W. von Bechterew, Professor in the University of Kasan. 8vo, pp. 210, with sixteen figures and a lithographed plate.

This is the authorised German version of a Russian treatise, mainly anatomical but partly physiological, dealing with the central nervous system. The translator is studying medicine at the Russian University of Dorpat. The author is the well-known neurologist and physician, recently promoted from Kasan to St. Petersburg.

The work consists of a clear if somewhat arbitrary statement of the anatomy of the human brain and spinal cord, as far as these are elucidated by the older methods of dissection, maceration, etc., assisted by the microscope, and especially as amplified by what is known as the Flechsig-Weigert method. Professor Bechterew has himself been an assiduous worker by this method, and has contributed a number of details to cerebral anatomy by its means; indeed the chief value of the volume before us is the outcome of this fact; and so preponderantly is the book compiled from the results obtained by this one method that it may be desirable in reviewing its pages to briefly recall the origin and principle on which the method is based.

The brain and spinal cord consist structurally of two kinds of anatomical substance, called from their tints respectively "grey matter" and "white matter". The nervous elements of the latter are without exception bundles of nerve fibres, to the fatty sheaths of which the opacity and milky appearance of the white matter is wholly due. That observant physiologist, the late Carl Ludwig, noted in the year 1872 that in the brain and spinal cord of new-born animals certain areas of the white matter are scarcely at all distinguishable from the grey matter. The want of whiteness in these areas proved, he found, to be due to the majority of the nerve fibres not having there at that early period of growth formed around themselves the sheathes of fat which they come later to possess. In this way originated the discovery that the nerve fibres of brain and spinal cord do not all develop together and at the same time and rate. Certain of them are late, certain of them earlier; and they develop in groups, late groups and early groups; and the grouping is determined strictly by function, fibres of identical function being developed contemporaneously.

The difference between certain groups or bundles in their date of acquirement of the fatty sheath is very great indeed. In the human embryo some nerve bundles are completely sheathed at half-term, others are not so even in children just old enough to walk. By following out these differences between sets of nerve fibres it is possible to trace the course of strands of them along the intricacies of the central nervous system; the method is much facilitated by use of dyes which stain deeply the fully-formed sheaths of the fibres, and leave the undeveloped sheath uncoloured. A remarkably successful method of dyeing for this purpose was devised by Weigert in 1883.

Ludwig put his discovery into the hands of his laboratory assistant, Paul Flechsig, and by the latter the anatomy of new important paths of conduction was made out in the years 1873-75, and published in a monograph. This research at once established Flechsig's fame, and led to his appointment as Professor of Psychioltrie in Leipzig. Even Flechsig's labours had not exhausted the new field of observation, and many details remained over for others to undertake. With several of these Bechterew occupied himself in Flechsig's laboratory in 1884-85. From this period arises the volume now under consideration.

The book is certainly an interesting one, but is at the same time disappointing. It bears many marks of having been written not at leisure, but rather in spite of the want of it.

Among the references not a few are erroneous; this is markedly the case with those from English sources (*cf.* pp. 25, 34, 201, etc.). Again it is remarkable that neither in the preface nor in the description of the methods introduced by Ludwig is Ludwig's name referred to. Again, the author, although he bases his dicta almost exclusively on Ludwig's developmental method, has not sufficiently considered the limits of delicacy and accuracy of that method: he does not bear in mind that although the method can trace with accuracy the course of compact bundles of fibres, it is of little use in tracing scattered sets of fibres from

one part of the brain to another. Further, and this is the chief defect of the volume, the author seems very imperfectly acquainted with the wide harvest of facts gathered in recent years by the delicate method of Wallerian degeneration. Such errors as occur in his description on pp. 16 and 17, for instance, are due to want of acquaintance with the Wallerian results. Finally, Professor Bechterew does not appear to have grasped the difference existing between atrophic degeneration (or the secondary atrophy of Gudden) and the Wallerian degeneration. The phenomena are fundamentally and absolutely distinct. The latter process is confined to the actual nerve cell upon which the lesion (mechanical injury or what not) is inflicted ; in the former phenomenon the field occupied is a whole physiological chain, or at least several links of a chain of functionally connected nerve cells.

When the work reaches, as we hope it may, a second edition, the author should bring it thoroughly up to date; the more so that in his preface he expresses his purpose in issuing the work to consist chiefly in the furnishing to busy students and practitioners a comprehensive account of the "Gegenwärtige Stand der Frage über die Leitungsbahnen des Hirns und mindestens den grössten Zeit der in dieser Hinsicht bekannten Thatsachen".

The figures in the text are very good, though very schematic. Several of them are original.

Movement. By E. J. Mary, Member of the Institute and of the Academy of Medicine, Professor at the College of France, Director of the Physiological Station. Translated by Eric Pritchard, M.A., M.B., B.Ch. (Oxon.). London : William Heinemann, 1895

The advances made in photography during the last few years have been very great, and in no direction have these advances been more remarkable than in the representation of movements which take place too rapidly to be followed by the unassisted eye. Professor Boys' photographs of flying projectiles, Dr. Worthington's of the splash of a drop, and others are illustrations of this. The visit of Mr. Muybridge to England some years ago and the exhibition of the results he had obtained by means of his ingenious apparatus brought prominently to the notice of the public, scientific and otherwise, the great strides which had been made in the analysis by means of photography of animal locomotion, and the appearance of Mary's book in France showed that considerable attention was being given to the subject in that country also.

Unfortunately in England the amateur photographer, to whom we owe almost all the advances that have been made in photography in recent years, is, as a rule, almost entirely ignorant of scientific matters outside his own subject, and therefore much talent is wasted for want of a definite object to follow out. It is therefore a subject for congratulation that a translation of Mary's work has appeared, and that it is well calculated to appeal to unscientific readers. In Mary's *Movement*, as in so many French scientific works, the style is admirably clear, and the explanations of even complicated pieces of apparatus such as to be easily understood. The scope of the work is wide—indeed it forms a good summary of the whole subject of the representation of movement by the graphic method assisted by photography. The measurement of space and time is treated in the preliminary chapters, and later on the application of these measurements and the assistance rendered by photography in such application to the elucidation of problems in movements of animate and inanimate objects. Locomotion in man, in quadrupeds, in fish, birds and insects is thus analysed.

In the case of the complicated rhythms of the locomotion of quadrupeds some difficulty is usually experienced in understanding the sequence of events, and it is therefore advisable to use every means in our power to render the representation of such movement as simple as possible. In some of the figures of horses in motion (*e.g.*, fig. 7, p. 9) it would have been simpler to have arranged the graphic records to read in the same direction as the horse is travelling in the figure, and in any case some indication should be given of the direction in which the record is to be read. The facts that the animal is travelling in one direction, that the record reads in the opposite direction, and that there is no indication of this, renders the figure puzzling to any one unused to the interpretation of graphic records.

In fig. 22 the description in the text does not correspond with that under the cut. In the former the *convex* surface of the card strip is to be blackened, in the latter the *inner* surface is said to be blackened.

Most of the figures, however, are well executed and judiciously selected, and are so numerous that even unscientific readers will find no difficulty in following the descriptions of the apparatus by means of which such excellent results have been obtained.

Of especial interest are the pages devoted to a description of the application of the method

to the study of microscopic forms of life, though in this department but little progress has as yet been made. In England several amateur photographers are turning their attention to this work, and we have hopes of soon seeing considerable advances made.

Altogether the book is an excellent summary of the subject, and the appearance of an English translation will, we hope, stimulate to fresh endeavours the numerous amateur photographers who at present seem to find a difficulty in selecting some subject to which to turn their attention. In the past, the most remarkable advances in photography have been due to amateurs ; in the future, we may expect equally brilliant results if only their efforts be properly directed. The subject treated in the book before us holds out great promises of success to any one taking up its study.

A Monograph of the Mycetozoa, being a descriptive catalogue of the species in the herbarium of the British Museum, illustrated with 78 plates and 51 woodcuts. By Arthur Lister, F.L.S. Printed by order of the Trustees, 1894.

Mr. Lister is well known as an authority upon the Myxomycetes, and his fine monograph, which appears under the auspices of the Trustees of the British Museum, will not only add lustre to the name of its author, but will prove a quite indispensable addition to the library of every one who is interested in this remarkable group of plants.

The introduction provides a useful general account of the organisms, whilst the diagnoses both of the main sub-divisions and also of the individual species are carefully and accurately executed, and to a great extent are based upon Rostafinski's work. Those species which, for various reasons, present special difficulties, are exhaustively dealt with, and the directions and limits of their individual variability are well defined.

We notice that here and there Mr. Lister has removed some forms from their old positions, and has located them in other genera. This is especially the case where the distinctive marks are perhaps not very obvious, and have thus been insufficiently appreciated by other writers less intimately familiar with the plants in question. Thus *Chondrioderma difforme* here reappears under the generic name of *Didymium*, with which it is certainly more appropriately classed.

Any notice of the book would be inadequate which omitted to mention the beautiful photographic illustrations which fill the 77 plates included in the volume. They are reproductions largely from Miss Lister's drawings, and of their wonderful fidelity, as well as of their beauty, it is impossible to speak in terms of too high praise.

The Cell : Outlines of General Anatomy and Physiology. By Dr. Oscar Hertwig. Translated by M. Campbell, and edited by Henry Johnstone Campbell, M.D. Swan Sonnenschein & Co., 1894.

The appearance of O. Hertwig's book, *Die Zelle und die Gewebe*, in 1893, marked an epoch in the progress of cytology. But so fast have new facts been accumulated that some of the statements and generalisations contained in the original work are to-day only of historical interest. It was then the more essential that a translator who proposed to edit such a book should himself be perfectly *au courant* with his subject.

But, as a matter of fact, we find many slips and inaccuracies in the volume which Dr. Campbell has given us, and some of these are not such as we should have expected to find in an "edited" translation. Thus, since the time at which Hertwig wrote, the generalisation as to the fusion of the centrosomes in conjugating cells has received several shocks before the present year, in which it seems to have received its final death-blow.

Again it seems odd that Dr. Campbell did not correct the errors which appear in the explanations of Figure 163, and which are equally made in the original. Furthermore, green swarm spores of algæ are *not* usually termed Tetraspores.

But in spite of these and similar weaknesses in the book regarded as an edition, it will doubtless meet with a warm welcome as presenting to English readers the best general account of the cell which has yet been written, and one, moreover, from the pen of an investigator in the very front rank of cytological research.

Protobasidiomyceten untersuchungen aus Brasilien. Von Alfred Möller, mit 6 Tafeln. Jena : Gustav Fischer, 1895.

Dr. Möller is to be congratulated on having produced not only a most important work on the group of fungi with which he here specially deals, but one which will be read from cover to cover with ever-growing interest. The Protobasidiomycetes are here welded into a coherent

whole, a feat only rendered possible by the extensive series of investigations carried out by the author within the Tropics, to which the majority of the plants appear to be confined.

Dr. Möller treats first of the Auricularia group, which is characterised by the transversely septate basidia, and he points out their relationship to the Uredineæ through such forms as *Saccoblastia*. He traces the evolution of the thallus from a mere encrusting form with scattered basidia, such as occurs in the genus *Stypinella*, on to the more complete types exemplified in *Auricularia* itself. Further, he finds in *Pilacrella* a prototype of the angiocarpic *Pilacre*, a plant which has been rendered familiar to us by the classical treatise of Brefeld. And similarly in the Tremellaccæ, in which the basidia are divided vertically crosswise, he finds a corresponding gradation of form from a simple type such as *Stypella* to the most complicated species of *Tremella*. Furthermore, *Pilacrella* finds its analogue in *Hyaloria*, which is practically a Pilacre with Tremelloid basidia.

The two groups, Auriculariaceæ and Tremellaceæ, are linked together by *Sirobasidium*, in which, besides other peculiarities, the basidia are usually divided obliquely by a septum. But both the position and number of the septa may vary.

Perhaps the most interesting generalisation in the book is that which emphasises the fact that amongst the Protobasidiomycetes there occur all the type forms which are met with in the Autobasidiomycetes, with the exception of that of the Agarics. It remains to be seen how far this is the result of independent convergence. Dr. Möller strenuously opposes the assumption of any direct phylogenetic relationship existing between the Proto- and Autobasidiomycetes, and he follows, or perhaps even exceeds, Brefeld, in regarding the two divisions as constituting two fundamentally distinct series. At the same time, it must be borne in mind that even amongst the Tremellas themselves there exists some latitude in the degree of completeness with which the septation of the basidia is carried out, and we are ourselves inclined to attach a good deal less weight to this character than Möller appears to do. To attempt to draw a hard and fast line of demarcation between the two groups of fungi on the strength of a single character, such as the absence or presence of septa, appears to us to be a dangerous proceeding, and one which savours rather too much of an *a priori* method. And this reflection obtrudes itself the more strongly when we take some of the author's own instances into consideration; for example, it is *solely* on this character that *Proto-merulius* is severed from *Merulius* (with which in every other respect it closely corresponds), and is regarded as belonging to a different original stock, —the resemblance, though striking, being merely one due to convergence.

The author elaborates Brefeld's view that the basidium is nothing save a specialised conidiophore which bears a definite number of spores. If, however, the recent conclusions drawn from the study of the nuclei within the basidia turn out to be sound, this comparison may require revision.

A further series of observations on the formation of the conidia in the Protobasidiomycetes is of special interest. Dr. Möller finds that many of these plants (*e.g.*, Pilacrella) possess two distinct forms of conidia occurring on the same mycelium. One of these, the large form, germinates readily; the other, the small kind, with difficulty or not at all. This discovery is of importance, since both amongst the Ascomycetes and the Uredineæ a similar dimorphism is common; the small conidia have been in these groups mistaken for sexual cells, but it is now generally admitted that this opinion was erroneous.

It is quite impossible, within the limits of a short review, to do justice to Dr. Möller's book and to the plates and figures which accompany the text. It is emphatically a work which every one who is at all interested in Fungi should read for himself.

The Structure of Man: An Index to his Past History. By Dr. R. Weidersheim, Professor in the University of Freiburg. Translated by H. and M. Bernard. The translation edited and annotated and a Preface written by G. B. Howes, F.L.S., Professor of Zoology, Royal College of Science, London. London: Macmillan & Co., 1895.

In placing the above work before the English-reading public the translators have fulfilled a useful task, for although, in spite of the short time it has been published, Professor Weidersheim's *Der Bau der Menschen* is already widely and favourably known amongst scientific workers, yet a large class to whom the present translation will be of great use are debarred from referring to the original by the fact of its being in the German language.

The class which above all others will benefit by the publication of an English translation embraces the younger human anatomists, to whom only too often the learning of the dry details of the structure of man proves uninteresting in the extreme, chiefly because as at present taught such details appear merely as a long array of facts without obvious connection, and

apparently without bearing on function. The medical student of to-day, though he spends far more time on so-called scientific study than his predecessors, yet is as a rule profoundly ignorant of two subjects which would tend to give a living interest to his anatomical studies, *viz.*, Embryology and Comparative Anatomy. Moreover, at the time he is learning his Anatomy he as a rule possesses only a superficial knowledge of Physiology. What possible interest then can he take in the study of Anatomy?

The writer of the present notice remembers with gratitude the help which the "Collective Investigation Committee of the Anatomical Society of Great Britain and Ireland" afforded him during his dissecting-room days. Under the stimulus which the search for "abnormalities" gave, all text-book statements were minutely investigated, and the discussion occasioned by the discovery of an unusual point of structure made an indelible impression on the memory, not only of the special abnormality present, but also of the more usual normal arrangement. With a work such as that of Professor Weidersheim to set forth such abnormalities and to describe and explain their significance, the impression made would have been even more profound.

Not only the human anatomist, however, but also the beginner in the study of comparative anatomy will find the book useful, for, in the words of the preface, "such works awaken the mind to the comparative method of inquiry, and to the higher educational and philosophic issues to which it leads".

Any detailed criticism of the subject-matter of the book would be out of place here, seeing that the original is already well known. It may, however, be well to state that amongst the contents sections are devoted to the Integument and the Tegumental Organs, the Skeleton, the Muscular System, Nervous System, Sense Organs, Alimentary Canal, and the Respiratory, Circulatory and Urinogenital Systems.

The translation throughout is especially well done, and, as is desirable in treating such a subject, the text is profusely illustrated by excellent figures.

By no means least amongst the advantages of the present edition must be ranked the suggestive preface and valuable notes contributed by Professor Howes.

The work should be widely known, as it serves to correlate the theories of modern science with the facts of every-day experience.

APPENDIX II.

CHEMICAL LITERATURE FOR NOVEMBER, 1895.

Vol. l. No. 300. *American Journal of Science.* (December, 1895.)

Penfield, S. L., Devices for the Separation of Minerals of high specific gravity (pp. 446-449). *Foote, W. M.*, Preliminary Note on a New Alkali Mineral (pp. 480-481).

Vol. xvii. No. 9. *American Chemical Journal.* (November, 1895.)

Jackson, C. L., and *Grindley, H. S.*, Action of Sodic Alcoholates on Chloranil. Acetals derived from Substituted Quinones (pp. 633-658). *Orndorff, W. R.*, and *Cameron, F. K.* Paranthracene or Dianthracene: A Polymeric Modification of Anthracene (pp. 658-681). *Campbell, E. D.*, On the Oxidation of some Gases with Palladinised Copper Oxide (pp. 681-692). *Kremers, E.*, Notes on Limonene and its Derivatives (pp. 692-697). *Wheeler, H. L.*, On the Action of Bromine on Metanitranilinc (pp. 697-704). *Kastle, J. H.*, A New Reagent for Bromine and Iodine. Preliminary Notice concerning the Halogen Derivatives of Sulphonamides (pp. 704-708).

Vol. xx. No. 237. *The Analyst.* (December, 1895.)

Bodmer, R., Note on a recent Milk Case involving a Sample of Abnormal Milk (pp. 265-268). *Bodmer, R.*, Note on Two Samples of "Filled" or Lard Cheese (p. 268). *Pearmain, T. H.*, and *Moor, C. G.*, The Composition and Analysis of Condensed Milk (pp. 268-274). *Allen, A. H.*, Note on Commercial Condensed Milks (pp. 274-276). *Williams, R.*, Note on the Iodine and Bromine Absorptions of Linseed Oil (pp. 276-277).

Vol. xiv. No. 11. *Journal of the Society of Chemical Industry.* (30th Nov., 1895.)

Dibden, W. J., The Filtration of Sewage Effluent (pp. 915-922). *Shack Sommer, G.*, Chemical Industries as Auxiliaries for the Practical Farmer (pp. 922-929). *Davis, G. E.*, A Quarter of a Century in the Heavy Chemical Trade (pp. 929-935). *Carulla, F. J. R.*, Chairman's Address (pp. 935-939). *Mackey, W. McD.*, Spontaneous Combustion of Oils spread on Cotton (pp. 940-941). *Clark, J.*, Chairman's Address (pp. 941-944). *Mond, L.*, The History of my Process of Nickel Extraction (pp. 945-946). *Alsberg, M.*, A Case of so-called "Spontaneous Combustion" (p. 947). *Enequist, J.*, The Utilisation of Waste Acid from the Parting of Gold and Silver (pp. 947-950).

Vol. xl. No. 247. *Philosophical Magazine and Journal of Science.* (Dec., 1895.)

Sutherland, W., Molecular Force and the Surface Tension of Solutions (pp. 477-494). *Thomson, J. J.*, The Relation between the Atom and the Charge of Electricity carried by it (pp. 511-544).

Series vii. Tome vi. *Annales de Chimie et de Physique.* (December, 1895.)

Lemoine, G., Études quantitatives sur l'action chimique de la lumière pour la décomposition mutuelle de l'acide oxalique et du chlorure ferrique (pp. 433-540). *Brillouin, M.*, Tensions superficielles et formes cristallines. Domaine d'action molèculaire (pp. 540-575).

Tomes xiii.-xiv. No. 22. *Bulletin de la Société Chimique de Paris.* (20th November, 1895.)

Thomas, M. V., Action de l'oxyde nitrique sur quelques chlorures métalliques : chlorures de bismuth et d'aluminium (pp. 1009-1010). *Lecoeuvre, F.*, Nouveau procédé volumétrique de dosage du nickel (pp. 1011-1013),

Tomes xiii.-xiv. No. 23. (5th December, 1895.)

Delépine, Préparation du nitrate de méthyle (pp. 1044-1045). *Matignon, C.*, et *Deligny*, Sur les substitution nitrées (pp. 1045-1048). *Urbain, G.*, Sur les produits de condensation de l'aldehyde isobutyrique (pp. 1048-1052). *Freundler, P.*, Dédoublement de l'acide dioxystéarique (pp. 1052-1055). *Freundler, P.*, Sur quelques éthers tartriques chlorés (pp. 1055-1063).

Tome cxxi. No. 20. *Comptes Rendus hebdomadaires de l'Académie des Sciences.* (11th November, 1895.)

Nivière, G., et *Hubert, A.*, Contribution à l'étude des ferments (p. 670). *Vigouroux*, Sur les

siliciures de nickel et de cobalt (pp. 686-689). *Dufau, E.*, Sur le chromite neutre de chaux cristallisé (pp. 689-691). *Lescoeur, H.*, Sur les alcoolates (pp 691-693). *Bourquelot, E.*, et *Hérissey, H.*, Sur les propriétés de l'emulsine des Champignons (pp. 693-696). *Winter, J.*, Constance du point de congélation de quelques liquides de l'organisme. Application à l'analyse du lait (pp. 696-698).

Tome cxxi. No. 21. (18th November, 1895.)

De Boisbaudran, L., Sur un élément probablement nouveau existant dans les terbines (pp. 709-710). *Phipson, T. L.*, Sur l'origine de l'oxygène atmosphérique (pp. 719-721). *Moureu, C.*, Synthèse du méthyleugénol. Constitution de l'eugénol (pp. 721-723). *Gérard, E.* Sur les cholestérines des Cryptogames (pp. 723-726). *Bertrand, G.*, et *Mallèore, A.*, Sur la diffusion de la pectase dans le règne végétal et sur la préparation de cette diastase (pp. 726-729).

Tome cxxi. No. 22. (25th November, 1895.)

Grimaux, C., Sur la para-èthoxyquinolcine (pp. 749-752). *Pichard, P.*, Dosage rapide de l'azote nitrique dans les produits végétaux (pp. 758-760). *Gouy*, Sur les propriétés électro-capillaires de l'acide sulfurique étendu (pp. 765-768). *François, M.*, Action du phénol sur l'iodure mercureux (pp. 768-771). *Vigouroux*, Sur le siliciure de manganèse (pp. 771-773). *Broginer, L.*, Sur la toxicité de l'acétylène (pp. 773-774). *Magnier de la Source, L.*, Sur quelques réactions de l'acide tartrique et des tartrates alcalins (pp. 774-776).

Tome cxxi. No. 23. (2nd December, 1895.)

Moissan, H., Sur la présence du sodium dans l'aluminium préparé par électrolyse (pp. 794-798). *Troost, L.*, et *Ouvrard, L.*, Sur l'origine de l'argon et de l'hélium dans les gaz dégagés par certaines eaux sulfureures (pp. 798-800). *Bouchard*, Observations relatives à la Communication de MM. Troost et Ouvrard (p. 800). *Moureu, C.*, Sur la présence de l'argon et de l'hélium dans une source d'azote naturelle (pp. 819-820). *Campredon, L.*, Détermination expérimentale du pouvoir agglutinant des Louilles (pp. 820-822). *Féree, J.*, Sur l'amalgame de chrome et quelques propriétés du chrome métalliques (pp. 822-825). *Colson, A.*, Sur un mode de synthèse d'amides complexes (pp. 825-827). *Guye, P. A.*, et *Goudet, C.*, Nouveaux exemples de superposition des effets optiques de carbones asymétriques (pp. 827-829).

15e Année. Tome ii. No. 10. *Journal de Pharmacie et de Chimie.*
(15th November, 1895.)

Berlioz et *Lépinois*, Examen d'un calcul amygdalien (pp. 433-435). *Bourquelot, E.*, et *Hérissey, H.*, Sur les propriétés de l'émulsine des champignons (pp. 435-440). *Chassevant, A.*, Action du benzène sur les microorganismes (pp. 440-442). *Grimbert, L.*, et *Choquet, J.*, Sur la présence du coli-bacille dans la bouche de l'homme sain (pp. 422-445).

15e Année. Tome ii. No. 11. (1st December, 1895.)

Charrin, A., Les Antitoxines (pp. 481-488). *Py*, Notes sur l'analyse des sucs de fruits, sirops et confitures (pp. 488-491).

Tome xxvii. No. 7. *Journal de la Société Physico-chimique Russe.*

Ipatieff, W., Action de l'acide bromhydrique sur les hydrocarbures $C_n H_{2n-2}$ (pp. 388-409). *Konowaloff, M.*, Action nitrifiante de l'acide nitrique sur les dérivés des hydrocarbures aliphatiques (pp. 409-412). *Konowaloff, M.*, Données concernment le pouvoir refringeant des combinaisons azotées (pp. 412-421). *Konowaloff, M.*, Action nitrifiante de l'acide nitrique sur les hydrocarbures aliphatiques (pp. 421-428). *Poehl, A.*, Sur un substance colorante de l'urine (pp. 428-431). *Schkateloff*, W., Sur l'identité de l'acide abiétique et de l'acide silvique (pp. 431-433). *Lidoff, A.*, Sur la composition de la mine d'asphalte de Kertsch (pp. 433-436). *Kutcheroff, M.*, Dosage de l'huile de fusel dans les alcools. *Kijner, N.*, Sur les métamorphoses des menthylamines droit et gauche. *Zelinsky, N.*, Sur le diméthylpentamethylène. *Scoupevsky, B.*, Sur le dosage des matières organiques dans l'eau au moyen du permanganate de potasse. *Tihvinsky, M.*, Sur les formes tautomériques des homologues de phényl-rosinduline. *Charitschkoff, C.*, Sur les methodes de l'analyse de la naphte (pp. 436-452).

Band cclxxxix. Heft 1. *Justus Liebig's Annalen der Chemie.*
(22nd November, 1895.)

Bredt, J., und *Rosenberg, M. V.*, Partielle Synthese des Kamphers und ueber die Constitution der Kamphersäure und des Kampherphorons (pp. 1-15). *Bredt, J.*, Ueber die von

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APPENDIX I.

NOTICES OF BOOKS.

Lectures on Human and Animal Psychology. By Wilhelm Wundt (translated from second German edition by J. E. Creighton and E. B. Titchener). London : Swan Sonnenschein, 1894.

The particular interest of this book is, that appearing so long ago as 1862, it is still in touch with the problems that are interesting us to-day as physiological psychologists. It is true that Professor Wundt rewrote it in great part in 1892, but the general trend of the work is the same as in the original, and it is very remarkable to see how what in 1862 was avowedly a programme is now in 1895 a working class-book of experimental psychology. Wundt himself says (preface to second edition) that "the physiological correlates of psychical processes" are less developed here than in the *Physiologischen Psychologie*, and that he devotes few words to subjects elsewhere discussed in detail, and gives more attention to others less prominent, especially in the last-named work. On this account the *Human and Animal Psychology* must not be taken as superseding the *Physiological Psychology* for English readers—hardly even as an introduction to it (as seems intended in the translators' preface). We should ourselves in preference turn to the larger work for a general presentation of any subject.

In view of the development of experimental psychology, it is interesting to note that Wundt has rewritten the chapters on Sensation, where laboratory work comes so much into evidence, while other more theoretical parts, *e.g.*, his theory of sense-perception by spatial localisation, have only undergone revision. And the *new* section is that on Will, Attention, Apperception, Association, so familiar to us all, as names, under Wundt's superscription. A short summary of the contents of the book, pausing at one or two points of present interest, may best indicate to what extent Wundt is the representative of "the new psychological movement" (translators' preface).

In 1862 it really was necessary to vindicate the claims of Psychology to be regarded as one of the natural sciences, independent of metaphysics. Wundt had to teach that not metaphysical questions, but "the uniformities of relation in the phenomena presented to our observation," are its subject-matter, and he was probably the first to assert that experimental psychology makes possible for philosophy that "absolutely impartial consideration of experience" in which alone metaphysical problems can hope for solution. On the other hand, he expressly points out that it is not merely chemical and physical investigation of the brain, but experimental observation of *the mind itself*, which must be the starting-point of psychology. [It will be remembered that Wundt uttered a strong protest last year (*Ueb. psychische Causalität*, etc., *Phil. Stud.*, xi.) against the "experimental psychology which reduces psychological processes to anatomical elements, until it almost appears as an appendix to muscular contraction".] Yet the nature of psychological facts is such that only the mental phenomena which are directly accessible to physical influences can be made the subject-matter of experiment, *i.e.*, the organs of sense and movement which are functionally related to mental processes. Hence it comes about that every psychological experiment is at the same time physiological, *and it is this new method of experiment which characterises the modern treatment of psychology.* The image of the external object or idea is the proper starting-point of a psychological analysis. Under Ideas, Wundt includes, (1) *sense-perceptions*, which depend upon direct excitation of the organs of sense ; (2) *memories of such sense-perceptions ;* (3) *images of fancy.* He protests against the separation of "ideas," as images of memory and fancy, from "perceptions," as direct effects of sense-impression. No essential psychological difference can be discovered between them, while sense-perceptions themselves might well be taken for illusions of sense. The first step in analysis must be to reduce the Idea to its simple constituent elements, or sensations. Sensations again have two properties, (*a*) *strength or intensity*, (*b*) *quality.* Neither of these can exist in the absence of the other, but they can be varied independently.

Starting with intensity we have first to find a measure of quantitative difference between sensations, and secondly the relation between stimulus and sensation. Wundt's account of Weber's Law, as formulating the latter, is clear and instructive, although based, in this work, on the least exact method of discrimination, *i.e.*, that of *minimum perceptible*

difference. The interpretation of the law may detain us for a moment, as the question it raises is still under discussion. Three interpretations are possible: (1) physiological, (2) psychological, (3) psycho-physical. Wundt himself subscribes to the second, *i.e.*, holds that the disproportion between increment of stimulus and increment of sensation takes place on *psychological* ground, between the sensificatory brain-charge, or internal stimulus, and the sensation. Delbœuf and others maintain that it is *physiological*, *i.e.*, that the internal stimulus is already a logarithmic function of the external objective stimulus, and that sensation increases in direct proportion with it. Confirmation of this from the physiological side appears in the recent observations of A. D. Waller (*Brain*, 1895, "Points relating to the Weber-Fechner Law, Retina; Muscle; Nerve") on muscle and retina, where, assuming the electrical signs of physico-chemical action recorded by galvanometer photographs to represent the sensificatory changes consequent on excitation of living matter, he finds that the internal change is always a logarithmic function of the external stimulus; the curves agree with the classical Weber-Fechner curve. (Wundt gives an excellent summary of the arguments on either side, up to 1893, in the fourth edition of his *Grundzuge d. physiol. Psychologie*, i., 390, where the position is much more clearly stated than in the work before us.)

In the absence of physiological evidence as to the relation of external to internal stimulus, Wundt had interpreted the Weber-Fechner Law by the psychological *Principle of Relativity*, finding "in the law of the logarithmic relation of sensation to stimulus a mathematical expression for a psychological process of universal validity". And under this principle he marshals all the remaining phenomena of sensation quantitatively apprehended, whether as regards intensity, or degree of quality. In our sense experience, we never deal with an isolated magnitude—there is always a process of comparison between the sum of previous experience, *i.e.*, former sensations, and the individual sensation of the moment. This principle is applied to the analysis of the several senses—sight and hearing being more fully treated. The physical data in either case are lucidly presented, and will be valuable to the student who has not had access to the general literature of the subject. The section on Contrast Phenomena opens up the Helmholtz-Hering controversy, *viz.*, whether contrast colours are due to subjective error of judgment, or to direct physiological interference with one another. Wundt prefers to denote the process underlying contrast as one of "associative contrast"; "contrast" there must be, on the principle of relativity, but "judgment" in his estimation belongs to a different category (*cf.* recent discussion in *Pflüger's Archiv*, xxxvii., xxxix., xl., xli., etc.).

How are Ideas built up from these sensational elements? Wundt next develops his theory of *sense-perception by spatial localisation*, analyses the mental processes of association in the several senses, and connects the separate elements of an association into a single idea.

Every sensation, when of sufficient intensity, and not inhibited by opposing influences, is followed by a muscular movement. Such movements we term reflex, and these reflexes gradually become purposive and definite (*e.g.*, light-stimuli at first excite merely irregular movements. After a time these take on a definite form, and serve a definite purpose; the eye moves in such a way as to bring the image of the stimulating light upon the yellow spot,—and if the light moves to and fro within the field of vision, the eye follows it with a continuous movement). The reflex thus controlled, and made to serve a definite purpose by the sensations inseparable from the movement itself, *muscle-sensations* being here singled out as illustrative. This brings us to another of the disputed questions in current psychology. Are the muscle-sensations (sense of effort, muscular sense) peripheral only, confined to muscle, skin, etc., or is there also central motor innervation? Here Wundt expresses himself with no reservation: our sensations of movement are by no means dependent solely upon the external or internal work performed by the muscles, but are influenced also by the intensity of the impulse to movement proceeding from the central organ in which the motor nerves originate. (This controversy, and the evidence deduced from the study of fatigue, the after-effect, and presumably correspondent of, muscular action, are admirably summed up in the *Physiol. Psychologie*, i., 426-434.) Leaving the question of the origin of sensations of muscular effort, Wundt finds pathological support of his conclusion that our recognition of the position of an object is normally based upon the sensation of effort attending the movement of the sensory organ to the object. For when muscular movements have been disorganised by paralysis, or other disturbances, there is often more or less complete re-adaptation to the new physiological conditions. So that if the power of localisation may be gained afresh after a transformation of the whole system of muscle-sensations we may assume that, when sense-perception in general was in process of development, the establishment of a relation between muscle-sensations, and the place from which an external stimulus operates, was a matter of slow and gradual evolution.

At the early stage now under consideration "purposive" and "definite" of course mean no more than the *simultaneous* or *successive association* of the sensations excited by the operation of a sense-stimulus. In the eye (to take the simplest case of muscular mechanism) association connects together the whole number of sensations excited in and round the eye, and systematises those sensory processes which begin with the simple light-sensation; it determines the form in which the eye transforms its sensations into perception. *This form is space-perception.* Hence Wundt infers that the perception of space, psychologically regarded, is not an innate possession of the mind, but the product of an association of sensations. This again is a crucial point in Wundt's psychology, in which (together with Helmholtz, and to some extent Lotze) he differs from Hering, who maintains that space-perception is innate and intuitive, and also from the English "Associationists". Wundt regards it as a mental growth depending on elementary experiences, tactile and visual, *e.g.*, in the latter on the action of the ocular muscles. We obtain an independent knowledge of space relations by the eye, not merely from inherited facts of past experience, although he admits of these that they form a certain psycho-physical "disposition" which abridges the process of individual space-construction.

And it is to the common action of the two organs of vision that we owe the greatest part of our ideational development. Binocular vision is of fundamental import. The fusion of the separate perceptions of the two eyes (*central fusion*, "the *mental resultant* of the originally separate perceptions") into a single idea is typical of the mental processes of association, as is the disorganisation which ensues when these perceptions cannot be co-ordinated.

Ideas are thus derived from sensations in the regular course of development. But the human mind never apprehends sensations with absolute indifference. In cognising we always "feel" attraction or repulsion, or again are incited to some kind of action. *Feeling* and *Will* are the accompaniment of ideas and sensations. No separation in the last resort is possible between sense and feeling.

We sense and we feel; sense-feeling is the affective tone of sensation, and is therefore among the elementary constituents of our mental life, while the higher intellectual and æsthetic feelings fall under the complex processes of consciousness. Will is not choice (which is a complex process, and implies the pre-existence of simple volition), nor is it the outcome of reflex action. Even the animal protozoa, much more the chick and the child, give plain evidence of voluntary movement. Purposive reflexes have much rather been evolved by practice from volitional actions. A *motive* is a particular idea with an affective tone attaching to it, and *motives* are the causes of volition, internal and psychical. In volition, however, as in sensation, the chief motive is not some particular sense-impression, but the entire trend of consciousness, as determined by previous experiences, often indeed "sub-conscious," not realised. It is the conflict of motives that gives to some voluntary actions the feeling of choice; where the present impression is very strong, and over-rules the rest, we have the sense of decision.

The *Theory of Volition*, shortly stated, is as follows:—

Feeling is independent of volition, but the reverse is impossible; voluntary activity always presupposes an antecedent voluntary tendency or feeling. Both these are elements of one common process. "*Activity*" implies two factors: (1) *a change that occurs;* (2) *a subject postulated* to explain the change.

In voluntary activity *the change* is always an alteration in the state of consciousness; an idea arises or disappears, becomes clear or obscure, etc. These ideational processes are invariably connected with feelings and emotions. In external voluntary actions the changes which refer to movements of the body (muscle-sensations, etc.) play the most important part. The *active subject*, which is the direct cause of these ideational changes, may be called "self," "ego," and again "willing subject," and so on, but in reality it is the sum of past experiences, *i.e.*, "consciousness". *Will* is therefore (*a*) *feeling;* (*b*) *change in ideational content;* (*c*) *dependence of this change upon the whole trend of consciousness.*

Consciousness is essentially a dynamic process. The mind is not a "stage," remaining when the "actors" have left—no idea once passed out of consciousness returns to it again. New processes arise which exhibit relations and similarity to previous processes, but are not identical with them. Nor is "consciousness" itself a mental fact, separable from its constituent processes; "in" and "out" of consciousness is a misnomer. It is *in having* the process that we are conscious of it,—the range of consciousness must be conceived as denoting simply *the sum of mental processes existing at a given moment.* But as a collective expression the concept is indispensable.

The range of consciousness at any moment can be worked out experimentally, *e.g.*, for auditory impressions. It is defined as the clear and distinct ideas apprehended by *apperception* together with the obscure ideas that are partly discriminated by *perception*. The *perceived* ideas lie in the field of consciousness ; the *apperceived* are situated at its fixation point and proceed *pari passu* with that of Self-Consciousness. The development of *Attention*, which is either active or passive. And as in the elementary mental processes, "feelings were found to be invariable forerunners and concomitants of volition," so here "ideas and the feelings that are connected with them serve as motives to the act of apperception, while apperception itself shows all the characteristics of an act of will. More than this, its two fundamental forms, the active and passive, obviously correspond to the two fundamental forms of conative activity, the passive form, the impulsive act, and the active, or act of choice.

"Now it is plain that these internal acts of will are not only the analogues of the external, but at the same time their condition. There can never be an external act save as the result of a previous inner selection, and this holds again both of the impulse and of the act of choice, *so that Apperception is the one original act of Will.*"

It is difficult to summarise the criticism directed against Wundt's Will and Apperception theories, because no two critics attack the same point, nor do they seem usually to know what it means, or what they mean by its meaning. Marty, *e.g.* ("Ueb. sprach Reflex," etc., *Viertel. jhr. schrft. f. Wiss. Phil.*, xiii., 1889), finds a confusion between Will and Affective Emotion, protesting that, while emotional were formerly classed under volitional reactions, Will is now proposed as subsidiary to the Emotions. Feeling is indifferently the stimulus of Will, and the reflex excited by it. The fact no doubt is, as Prof. Titchener elsewhere expresses it (*Mind*, i., n. s., 1892), that in Wundt's psychology, the words "will" and "apperception" have a two-fold meaning: (1) as indicating a primitive mental activity, (2) as expressing the complex states derived from this in different directions.

The best discussion is that of Külpe in *Phil. Stud.*, v., 1889. He groups the rival theories broadly into two classes—negative and positive. The former, or "physiological" school, of which Münsterberg is the chief exponent, deny that Will is an elementary constituent of Consciousness, and reduce it to a derivative of sensation and feeling: the other, the "psychological" group, headed by Wundt, take the strictly empirical view that Will, like sensation and feeling, is a primitive element. Külpe adheres to the latter, and insists that, while some psychologists seem to shrink from reckoning spontaneous activity among the facts of consciousness, there must still remain, when we have separated out ideation, sensation and feeling, an independent qualitative factor which is best described as internal spontaneous activity.

It is, perhaps, legitimate to ask why this fundamental activity should be termed "Will," unless will is identical with function—as it is in the last resort. But the precise terms must always be ambiguous. The burning point that enforces itself upon us is Wundt's conception of Consciousness as a relation, a synthesis—a summated whole, and not a system of detached compartments, or worse, a "background," as it is still unfortunately described by some writers.

The same section treats of the rhythm or fluctuation of Attention, which has recently been the subject of so much physiological experiment and argument. Wundt, of course, refers it to a central periodicity of the apperceptive process, to central oscillations of attention, while his opponents regard it as dependent on purely physiological conditions in the peripheral sense-organs and muscles (Münsterberg, G. E. Müller).

Reaction-times are discussed as the method for registration and measurement of mental processes ; a more rapid reaction is obtained in the sensorial form (where the sense-organ is attended to) than in the muscular (where attention is directed to the organs of movement) (Titchener, *cf. Phil. Stud.*, viii.).

Association-times furnish another interesting section. Wundt finds that a single letter takes from ten to twenty seconds more to cognise in German than in Roman type, whereas the entire word can be read with as great facility in the one as in the other. In the same way one finds on plunging into the life of a foreign town that familiar subjects are understood almost at once, while it takes an immense time to grasp the immediate sense of an unexpected topic.

Simultaneous Association is the simplest form, in which the associated ideas are not successive, but come into consciousness as a simultaneous ideational complex, connected by their mutual relations in consciousness without any direct implication of memory.

In *Assimilation* the memory image takes the lead, and presides over the incoming sensations, perhaps forming a wrong concept ; indistinct sounds, *e.g.*, may be represented by a wrong set of memory ideas. Hence the familiar beaker experiment, and other sense illusions, due to *delay* in assimilation of new sense impressions by the habitual memory

elements, and sense illusions proper where the ideas excited by association permanently outweigh the direct sense impressions. Next comes *Successive Association*, (1) immediate, (2) mediate, a section which Wundt is constantly elaborating ; *cf. Phil. Stud.*, x., 2, where he refers the experience of an idea, or memory image, called up by the unnoticed scent of a flower, quoted by Jerusalem as a case of an "unconscious" middle factor, to the category of an unrecognised, but *not unconscious*, factor—dimly perceived, but not apperceived in consciousness.

He next discusses the "intellectual, conscious processes," which include *concepts, judgments, and apperceptive connections of ideas.*

The complex affective processes are summarised as Emotions, and the expression of the Emotions—Instinctive and Voluntary Action—with a concluding chapter on Immortality, Phrenology, and the Principles of Psycho-physical Parallelism. It is impossible in the limits of a review to give a competent outline of the contents of this book, one falls between the catalogue and the encyclopædia. The physiological sections are the most convincing, and command respect for the profundity and grasp of Wundt's treatment in comparison with the superficial handling of some later writers.

A translation from the hands of Prof. Titchener carries its own recommendation. He is so strong on the point of "psychological nomenclature" that he doubtless has good reasons for introducing such unfamiliar terms as "clang," "clang character," "clang compound," "*deception* of judgment," "just noticeable difference," etc. But in lecture vi. it is difficult to subscribe to the rendering of "farblos" by "bright," "brightness" (throughout the chapter). Why not *a-chromatic?* The division into numbered sections is a distinct help towards acquaintance with the subject-matter.

F. A. WELBY.

An Introduction to the Study of Seaweeds. By George Murray, F.R.S.E., F.L.S. London: Macmillan & Co., 1895.

Mr. Murray's name is well known in connection with Algæ from his researches published in *Phycological Memoirs* (of which he is editor), his *Catalogue of the Marine Algæ of the West Indian Region*, and other works.

The book before us opens with an introduction of thirty-three pages, dealing shortly with the history of Phycology, with geographical distribution of seaweeds and the causes affecting it, and includes hints on collecting. This is followed by a short Bibliography ; and the remainder of the volume is occupied with the general morphology of the seaweeds.

For reasons of convenience the Phæophyceæ are described first, but it seems rather a pity that the sub-classes should be numbered in the order adopted, *viz.*, Phæophyceæ, Chlorophyceæ, Diatomaceæ, Rhodophyceæ, Cyanophyceæ.

The results of some of the researches recently published in the *Phycological Memoirs* are incorporated in the descriptions, and several of the genera described and figured are uncommon ones, and information about them difficult to get. These features, and also the large number of excellent illustrations, contribute largely to the value of the book. There are eighty-eight figures, representing habit, microscopical structure, etc., besides eight coloured plates. With these numerous illustrations it is hardly fair to ask for more, but one would have liked *one* figure of the conjugation of planogametæ, to compare with the fertilisation of Fucus.

The Bangiaceæ are, no doubt rightly, excluded from the Rhodophyceæ (p. 248), but a comparison of *Porphyra* with *Prasiola* might, perhaps, have been added by way of a suggested relationship.

The descriptions are mostly concise, but a few statements are made in rather too general terms to be of much service ; thus (on p. 113, last line), some of the Sphacelariaceæ, when attacked by Chytridiaceæ, "present appearances which have been a source of error in their interpretation". This rouses one's curiosity without gratifying it, but it is perhaps useful as an incentive to research. The following also seems rather too vague to be suitable for a manual for students: "A certain resemblance of this remarkable body to the Volvocineæ (fresh water) suggests inevitable speculations which may easily be made too much of".

In perusing the book one comes across occasional misprints, which will, no doubt, be corrected in a future edition. Thus, on page 190, the size of unusually large Diatoms is given as a third of a micro-millimetre for fresh water forms, and about three micro-millimetres for marine forms. Taking a micro-millimetre in the sense in which it is used by botanists and zoologists, this would make the length of these Diatoms about a hundredth part of the real value.

A book on Algæ has long been a desideratum, and all students will feel indebted to the author for removing many of the difficulties which have hitherto hedged round the study of some of the most interesting groups of the Vegetable Kingdom.

H

APPENDIX II.

CHEMICAL LITERATURE FOR DECEMBER, 1895.

Vol. i. No. 1. *American Journal of Science.* (January, 1896.)

Wells, H. L., and *Foote, H. W.*, Double Fluorides of Caesium and Zirconium (pp. 18-21). *Gooch, F. A.*, and *Peirce, A. W.*, Iodometric Determination of Selenious and Selenic Acids (pp. 31-35). *Browning, E. P.*, Interaction of Chromic and Arsenious Acids (pp. 35-38).

Vol. xvii. No. 12. *Journal of the American Chemical Society.* (Dec., 1895.)

Wait, C. E., A Convenient Still for the Laboratory (pp. 917-920). *Trubek, M.*, On the Estimation of the Extraction in Sugar Houses (pp. 920-923). *De Chalmot, G.*, On Silicides of Iron (pp. 923-925). *Williams, C. B.*, Estimation of Phosphoric Acid in Soils by Double Precipitation with Molybdic Solution, and Titration of the Ammonium Phosphomolybdate with Standard Alkali (pp. 925-927). *Walker, P. H.*, On the Use of Organic Bases in the Preparation of Barium and Calcium Ferrocyanides (pp. 927-932). *Linebarger, C. E.*, On the Formation of Layers in Mixtures of Acetic Acid and Benzine (pp. 932-933). *Hyde, F. S.*, Scheme for the Identification of Acetanilide, Phenacetine, Quinine Sulphate, etc. (pp. 933-935). *Blasdale, W. C.*, On the Physical and Chemical Properties of Some California Oils (pp. 935-941). *Kilgore, B. W.*, The Determination of Phosphoric Acid by the Molybdate-Magnesia Method and by Volumetric Methods (pp. 941-943). *Auchy, G.*, The Volumetric Estimation of Manganese (pp. 943-947). *Venable, F. P.*, An Early Arrangement of the Elements (pp. 947-950). *Kilgore, B. W.*, On the Estimation of Phosphoric Acid by Titration of the Ammonium Phosphomolybdate Precipitate with Standard Alkali (pp. 950-969). *Dudley, W. L.*, The Relationship of the Heat of Vaporisation of Gases to their Density and also to their Boiling Point (pp. 969-986). *Gill, A. H.*, and *Hunt, S. P.*, The Determination of Methane and Hydrogen by Explosion (pp. 986-994).

Vol. xvii. No. 10. *American Chemical Journal.* (December, 1895.)

Mabery, C. F., On the Composition of the Ohio and Canadian Sulphur Petroleums (pp. 713-748). *Stillman, J. M.*, and *Yoder, M. B.*, On the Combination of Anhydrous Ammonia and Aluminium Chloride (pp. 748-754). *Schlundt, H.*, and *Warder, R. B.*, The Chemical Kinetics of Oxidation. 1. On the Speed of the Liberation of Iodine in Mixed Solutions of Potassium Chlorate, Potassium Iodide and Hydrochloric Acid (pp. 754-770). *McCay, W. Le Roy*, On the Existence of the Sulphoxyantimoniates (pp. 770-779). *Freer, P. C.*, Tetrinic Acid (pp. 779-796).

Vol. lxvii. No. 397. *Journal of the Chemical Society.* (December, 1895.)

Henderson, G. G., and *Prentice, D.*, The Action of certain Acidic Oxides on Salts of Hydroxy Acids. II. (pp. 1030-1040). *Dixon, A. E.*, Acidylthiocarbimides (pp. 1040-1049). *Smithells, A.*, On Flame Temperatures and the Acetylene Theory of Luminous Hydrocarbon Flames (pp. 1049-1063). *Young, G.* Synthesis of Diphenyloxytriazole (pp. 1063-1071). *Thomas, G. L.*, and *Young, S.*, The Vapour Pressures, Specific Volumes, and Critical Constants of Normal Hexane (pp. 1071-1084). *Perkin, A. G.*, Some Constituents of the Root of *Polygonum Cuspidatum* (pp. 1084-1090). *Chattaway, F. D.*, and *Ingle, H.*, A New Series of Hydrazines (pp. 1090-1095). *Divers, E.*, and *Haga, T.*, Sodium Nitrososulphate (pp. 1095-1098). *Divers, E.*, and *Haga, T.*, The Constitution of the Nitrososulphates (pp. 1098-1105). President's Address (pp. 1105-1172).

Vol. lxix. No. 398. (January, 1896.)

Perkin, W. H., Influence of Temperature on the Refractive Power and on the Refraction Equivalents of Acetylacetone and of Ortho- and Para-toluidine (pp. 1-7). *Meldola, R.*, and *Andrews, E. R.*, The Alkaline Reduction of Meta-nitraneline (pp. 7-17). *Dixon, A. E.*, The Chemistry of Dibromopropylthiocarbimide and the Action of Bromine and of Iodine

on Allylthiourea (pp. 17-36). *Forster, M. O.*, Studies of the Terpenes and Allied Compounds. New Derivatives from α-Dibromocamphor (pp. 36-61). *Kipping, F. S.*, ω-Bromocamphoric Acid (pp. 61-66). *Paterson, D.*, Efflorescence of Double Ferrous Aluminium Sulphate on Bricks exposed to Sulphur Dioxide (pp. 66-68). *Schunck, E.*, and *Marchlewski, L.*, Some Derivatives of Anthraquinone (pp. 68-74).

Vol. xiv. No. 12. *Journal of the Society of Chemical Industry.*
(31st December, 1895.)

Hargreaves, J., Alkali-Manufacture by the Hargreaves-Bird System of Electrolysis (pp. 1011-1017). *Rideal, S.*, and *Rosenblum, S.*, The Analysis of Chrome Iron Ore, Ferro-Chromium and Chrome Steel (pp. 1017-1019). *Bailey, G. H.*, The Volatilisation of Salts during Evaporation (pp. 1020-1021). *Pattinson, J.*, and *Pattinson, S. H.*, Note on the Estimation of Phosphorus in Iron and Iron Ores containing Titanium (p. 1022). *Hogg, T. W.*, A Note on Colorimetry (pp. 1022-1023). *Shaw, S.*, Deposits found in Old Workings at Delaval Benwell (Northumberland) and Tanfield Lea (Durham) Collieries (pp. 1023-1024). *Clowes, F.*, The Composition of the Limiting Explosive Mixtures of Combustible Gases with Air (pp. 1024-1025). *Procter, H. R.*, and *Towse, W.*, Note on the Saline Earth used in Curing Hides in India (pp. 1025-1026). *Perkin, A. G.*, Note on a Method of Subliming Alizarine (pp. 1026-1027). *Proude, J.*, and *Wood, W. H.*, On the Cause of the Change in Colour of Soaps containing Indigo Extract (p. 1027). *Proude, J.*, and *Wood, W. H.*, On the Solubility of Indigo in Oils (pp. 1027-1028). *Tervet, R.*, Destructive Distillation (pp. 1028-1030). *Schweitzer, H.*, and *Lungwitz, E.*, The Iodine Number of Fats and Oils (pp. 1030-1035).

Vol. xli. No. 248. *The Philosophical Magazine and Journal of Science.*
(January, 1896.)

Griffiths, E. H., and *Dorothy Marshall*, The Latent Heat of Evaporation of Benzene (pp. 1-38). *Dorothy Marshall*, and *Ramsay, W.*, A Method of Comparing Directly the Heats of Evaporation of Different Liquids at their Boiling Points (pp. 38-52). *Appleyard, R.*, A Direct-reading Platinum Thermometer (pp. 62-72).

Tome vii. *Annales de Chimie et de Physique.* (January, 1896.)

Berthelot, Recherches sur l'argon et sur les combinaisons (pp. 5-27). *Berthelot*, Sur la combinaison de l'azote libre avec les éléments du sulfure de carbone (pp. 27-29). *Berthelot* et *Rivals*, Nouvelles recherches sur les relations thermochimiques entre les aldéhydes, les alcools et les acides (pp. 29-47). *Berthelot* et *Rivals*, Sur les lactones ou olides camphoréniques (pp. 47-50). *Berthelot*, Sur les chaleurs de dissolution et de neutralisation des acides camphoréniques (pp. 50-51). *Berthelot*, Relations thermochimiques entre les états isomeriques du glucose ordinaire (pp. 51-58). *Carvallo, E.*, Absorption de la lumière par les cristaux (pp. 58-94). *Meslans, M.*, Sur les vitesses d'éthérification de l'acide fluorhydrique (pp. 94-112). *Moissan, H.*, Analyse de quelques échantillons de chandôo et étude chimique de la fumée d'opium (pp. 112-125). *Wunsch. A.*, Sur la benzoïlquinine (pp. 125-144).

Tomes xiii.-xiv. No. 24. *Bulletin de la Société Chimique de Paris.*
(20th December, 1895.)

Varet, R., Recherches sur les combinaisons du cyanure de mercure avec les sels halogénés (pp. 1076-1092). *Brizard, L.*, Sur quelques sels d'Argent du ruthénium nitrosé (pp. 1092-1093). *Vaillant, V.*, Action du chlorure de carbonyle sur la dithioacétylacétone (pp. 1094-1095). *Bertrand, G.*, Sur la recherche et la présence de la laccase dans les végétaux (pp. 1095-1098). *Manceau, E.*, Sur le dosage du tannin dans les vins (pp. 1098- 1101). *Winter, J.*, Température de congélation des liquides de l'organisme. Application à l'analyse du lait (pp. 1101-1107).

Tomes xv.-xvi. No. 1. (5th January, 1896.)

Jaubert, F. G., Nouvelle turbine et agitateur pour laboratoire (pp. 9-10). *Brochet, A.*, Action du chlore sur l'alcool propylique normal (pp. 10-16). *Brochet, A.*, Influence de l'acidité sur les oxydations d'alcools (pp. 16-20). *Brochet, A.*, Action du chlore à chaud sur l'alcool isobutylique (pp. 20-22). *Collet, A.*, Sur la triphényléthanone (pp. 22-23). *Grimaux, E.*, Sur la paraéthoxyquinoléine (pp. 23-25). *Béhal, A.*, et *Blaise, E.*,

Action de l'hypoazotide sur l'acide campholénique (pp. 25-31). *Bertrand, G.*, et *Mallèvre, A.*, Sur la diffusion de la pectase dans le règne végétal et sur la préparation de cette diastase (pp. 31-33). *Jay, H.*, Sur la dissémination de l'acide borique (pp. 33-39). *Lucas, M.*, Dosage colorimétrique du plomb (pp. 39-42). *Joffre, J.*, Nouvelles recherches sur la valeur agricole du phosphate rétrogradé (pp. 42-46).

Tome cxxi. No. 24. *Comptes Rendus hebdomadaires de l'Académie des Sciences.* (9th December, 1895.)

Moissan, H., Analyse de l'aluminium et de ses alliages (pp. 851-856). *Amagat, E. H.*, Sur les variations du rapport des chaleurs spécifiques des fluides. Acide carbonique (pp. 863-866). *Lechartier, G.*, De l'analyse du sol par les plantes (pp. 866-870). *Deslandres, H.*, Absorption de l'azote par le lithium à froid (pp. 886-887). *Lemb, C.*, Sur un procédé possible de séparation de l'argon et de l'azote atmosphériques (pp. 887-888). *François, M.*, Action de l'alcool sur l'iodure mercureux (pp. 888-891). *Prud'homme, M.*, Nouvelle synthèse de la parafuchsine et de ses dérivés mono-, di-, tri-, et tétra-alcoylés (pp. 891-893). *De Coninck, O.*, Sur un mode de décomposition de quelques corps organiques à fonction amide et imide (pp. 893-894). *Coquillion, J.*, Sur les limites d'approximation que peut donner le grisoumètre à fil de platine ou de palladium dans les dosage du gaz formène (pp. 894-896). *Jay, H.*, Sur la dispersion de l'acide borique dans la nature (pp. 896-899). *Dastre, A.*, Solubilité et activité des ferments solubles en liqueurs alcooliques (pp. 899-901).

Tome cxxi. No. 25. (16th December, 1895.)

Girard, A., Composition des farines et issues fournies par la mouture aux cylindres de blés tendres et blés durs (pp. 922-929). *Rossel, A.*, Combinaison directe de l'azote de l'air atmosphérique aux métaux, sous forme d'azoture de magnésium, d'aluminium, de fer, de cuivre, etc. (pp. 941-943). *Mourlot, A.*, Sur la préparation et les propriétés du protosulfure de chrome cristallisé (pp. 943-945). *Guntz*, Sur le sous-chlorure de lithium (pp. 945-947). *Jaubert, G. F.*, Sur quelques nouvelles safranines (pp. 945-947).

Tome cxxi. No. 26. (23rd December, 1895.)

Allocution de M. Marey (pp. 969-979).

Tome cxxi. No. 27. (30th December, 1895.)

Hardy, E., Sur l'analyse acoustique des mélanges de deux gaz de densités différentes (pp. 1116-1117). *Solvay, E.*, Sur la production mécanique des températures extrèmes (pp. 1141-1143). *Cailletet*, Observations au sujet de la Communication précédente (pp. 1143-1144). *Le Chatelier, H.*, Sur la combustion de l'acétylène (pp. 1144-1147). *Maquenne, L.*, Sur la fixation de l'azote par les métaux alcalino-terreux (pp. 1147-1148). *Levy, L.*, Sur le titane cristallisé et les combinaisons de titane et de silicium (pp. 1148-1150). *Gernez, D.*, Sur le pouvoir rotatoire de la rhamnose (isodulcite) surfondue (pp. 1150-1152). *Lauth, C.*, Sur quelques dérivés dithiazoliques (pp. 1152-1155). *Colson, A.*, Synthèses de chlorhydrates d'amides et de chlorures d'acides (pp. 1155-1156). *Brochet, A.*, Action des halogènes sur l'aldéhyde formique (pp. 1156-1159). *Barbier, P.*, et *Bouveault, L.*, Sur l'essence de Lémon grass (pp. 1159-1162). *Rey-Pailhade, de, J.*, Rôles respectifs du philothion et de la laccase dans les graines en germination (pp. 1162-1164). *Sidersky, D.*, Sur la détermination simultanée de l'acidité minérale et organique dans le jus de betteraves (pp. 1164-1165).

16 Année. Tome iii. No. 1. *Journal de Pharmacie et de Chimie.* (1st January, 1896.)

Mouren, C., Sur la présence de l'argon et de l'hélium dans une source d'azote naturelle (pp. 5-6). *Meillère, G.*, Dosage de l'acide carbonique libre et de l'acide carbonique combiné dans les eaux bicarbonatées (Vichy-Vals) (pp. 6-8). *Lépinois, E.*, Contribution à l'étude ed de l'acidité urinaire (pp. 8-16). *Halphen, G.*, Sur la recherche des huile végétales ou animales dans les huiles minérales (pp. 16-19). *Magnier, L. de la Source L.*, Sur quelques réactions de l'acide tartrique et des tartrates alcalins (pp. 19-21). *Balland*, Sur la répartition des matières azotées et des matières minérales dans le pain (pp. 21-24).

Tome xxvii. No. 8. *Journal de la Société physico-chemique russe.*

Konowaloff, M., Sur une réaction sensible pour reconnaître les composés nitrés primaires et secondaires (pp. 453-456). *Konowaloff*, Sur les phénomènes de l'isomérie ayant lieu lors

de la synthèse des hydrocarbures aromatiques par la méthode Friedel-Crafts (pp. 456-459). *Kijner, N.*, Sur les amines et les hydrazines de la série polyméthylénique (pp. 459-492). *Tchernik, G.*, Analyse de l'or du district de Batoum (pp. 492-496). *Charitschkoff, K.*, Sur les méthodes d'analyse de la naphte (pp. 496-500). *Scoupevsky, B.*, Sur le dosage des matières organiques dans l'eau au moyen du permanganate de potasse (pp. 500-506). *Mendeleeff, D.*, Sur l'argon et le hélium. *Mendeleeff, D.*, Méthode des pesées precises. *Dobrohotoff, A.*, Dosage de l'acide carbonique dans l'air d'une chambre. *Volpian, L.*, Sur la structure du cymol naturel. *Mokievsky, W.*, Sur l'isoprène de l'huile de térébenthine. *Chaternikoff, M.*, et *Setchénoff, J.*, Appareil pour analyse des gaz. *Bogorodsky, A.*, Sur les cryohydrates. *Tichvinsky, M.*, Sur les rélations des safranines et des indulines. *Reformatsky, S.*, et *Pletskonoff, W.*, Action du zinc et de l'ether bromisobutyrique sur l'acéton. *Reformatsky, S.*, Actions du zinc et de l'ether bromisobutyrique sur l'aldehyde isobutyrique. *Barilowitsch, A.*, Sur l'acide düsopropyloxalique (pp. 506-520).

Tome xiv. No. 4. *Recueil des Travaux Chimiques des Pays-Bas.*

Franchimont, A. P. N., et *van Esp, H.*, Contribution à la connaisance des nitramines (pp. 235-252). *Hoogewerff, S.*, et *Van Dorp, W. A.*, Sur quelques dérivés de l'acide camphorique et de l'acide hemipinique (pp. 252-276).

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Knoevenagel, E., Synthesen in der Kampher und Terpenreihe (pp. 131-173). *Lipp, A.*, Synthese vierfach hydrirter Pyridenderivate und Ueberführung derselben in Piperidenverbindungen I. (pp. 173-254). *Markovnikoff, V.*, Ein Mischapparat zur Beschleunigung chemischer Reactionen (pp. 254-257).

Band ccxxxiii. Heft 9. *Archiv der Pharmacie.* (8th December, 1895.)

Gohlich, W., Ueber Morphin und Morphinhydrochlorid (pp. 631-646). *Gadamer, J.*, Ueber das Thiosinamin (pp. 646-684). *Hesse, O.*, Ueber die Bestandthiele von Aristolochia argentina (pp. 684-698). *Kiliani, H.*, Zur Kenntnis des Digitalinum verum (pp. 698-700).

Band lii. Nos. 22, 23. *Journal für praktische Chemie.* (28th November, 1895.)

Curtius, T., Hydrazide und Azide organischen Säuren X. *Radenhausen, R.*, Ueber Hydrazide substituirter Amidosäuren und das Hydrazid der Fumarsäure (pp. 433-454). XI. *Curtius, T.*, und *Heidenreich, K.*, Die Hydrazide und Azide der Kohlensäure (pp. 454-490). *Kumahow, N.*, Ueber die zusammengesetzten Metallbasen II. (pp. 490-528).

Band lii. No. 24. (14th December, 1895.)

Claus, Ad., Zur Kenntniss der Tetramethylbenzoësäuren (pp. 529-532). *Claus, Ad.*, und *Howitz, H.*, Einwirkung von Brom auf p- und o-Oxychinolin (pp. 532-548). *Vaubel, W.*, Der Benzolkern. V. (pp. 548-555). *Schneider, R.*, Notiz ueber die Zusammensetzung und die Constitution des Cubans (pp. 555-559). *Elbs, K.*, und *Brunnschweiler, E.*, Eine einfache Darstellungsweise des Trichlortoluchinons (pp. 559-560).

Jahrgang xxxv. Heft 1. *Zeitschrift für Analytische Chemie.* (11th November, 1895.)

Dennstedt, M., und *Ahrens, C.*, Ueber die Bestimmung von schwefliger Säure und Schwefelsäure in den Verbrennungs-producten des Leuchtgases (pp. 1-10). *Kippenberger, C.*, Die Benutzung von Jodlösungen zum Zweck der titrimetrischen Werthbestimmung von Alkaloidlösungen (pp. 10-27). *Riegler, E.*, Die Bestimmung des Alkohols und Extractes im Weine auf optischem Wege (pp. 27-31). *Riegler, E.*, Eine Methode zur Bestimmung der Harnsäure, beruhend auf der Eigenschaft, Fehling's Lösung in der Wärme zu rothem Kupperoxydul zu reduciren (pp. 31-35). *Bornträger A.*, Zur Prüfung des Bergamottöles auf Reinheit (pp. 35-38). *Rössing, A.*, Mittheilungen ueber das Schwarzwerden der Gemüseconserven in Weissblechdosen (pp. 38-50).

Band xi. Heft 1. *Zeitschrift für Anorganische Chemie.* (30th December, 1895.)

Sörensen, S. P. L., Kritische Präparatenstudien (pp. 1-6). *Crookes, W.*, Das spektrum des Heliums (pp. 6-14). *Thomsen, J.*, Experimentelle Untersuchung ueber das Atomgewichtsverhaltnis zwischen Sauerstoff und Wasserstoff (pp. 14-31). *Hofmann, K. A.*, Ueber das Nitroprussidnatrium II. (pp. 31-37). *Jannasch P.*, und *Weingarten, P.*, Ueber die

quantitative Bestimmung des Wassers in Silikaten nach der Boraxmethode (pp. 37-40). *Jannasch, P.*, und *Weingarten, P.*, Ueber die chemische Zusammensetzung und konstitution des Vesuvians und des Wiluits II. (pp. 40-49). *Funk, R.*, Ueber den Schwefel und Kohlenstoffgehalt der Zinks (pp. 49-59).

Band xviii. Heft 4. *Zeitschrift für Physikalische Chemie.* (27th December, 1895.)

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